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A SCIENTIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION, THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

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The Journal *of* Parapsychology

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Parapsychology

Volume 16 JUNE, 1952 Number 2

EDITORIAL

EDITORIALS, A FORUM, AND A SYMPOSIUM

THE PAGING of papers scheduled for this issue has not left enough space for the usual, more discursive editorial. We are therefore taking advantage of this opportunity to offer a few comments on policy and program—to explain the general principles underlying our editorial articles and to review decisions concerning the parapsychology forum and the new symposium which were proposed in our December number.

On the question of editorials we are governed by the judgment that if it is necessary to have a JOURNAL, it is necessary to have an editorial viewpoint and policy. Furthermore, such a policy should be as explicit as possible so that readers may judge the background of thought against which the issues raised by specific papers are being considered. What is needed, then, to achieve this purpose is a type of editorial that records the general perspective accompanying the selection and editing of the articles that appear in the JOURNAL and the rejection of the ones that do not.

We do not assume that the general editorial background that will be portrayed will be perfect, or balanced, or adequate, or anything else in the way of an absolute criterion. It is to be regarded as merely the staging for the current scene so far as the main JOURNAL articles are concerned, leaving the responsibility for the specific content primarily, of course, with the author.

Our precedent was set by the editorial contributed to the very

first issue in March, 1937, by the senior editor at that time, the late Professor William McDougall, F.R.S. It has been something of a model for us ever since.

The comments received from readers in the past have encouraged us to continue the general editorial and this we propose to do, although not, perhaps, in every issue of the JOURNAL.

To bring in other viewpoints and to help adjust our perspective, we raised in the December number the question of the advisability of having a section to be known as the Parapsychology Forum, a section to be given over to a discussion of debatable issues from different points of view, the answering of questions, and the expression of reader opinion. Readers' comments and further reflection over the need for a forum have led us to decide in favor of giving this plan a fair period of trial and to do this in spite of the added editorial demands it will make on an already burdened staff.

The question has arisen, however, as to the propriety of adding this section to the JOURNAL OF PARAPSYCHOLOGY, and the suggestion has been made that the JOURNAL'S smaller affiliate, the *Parapsychology Bulletin,* be expanded to include the Forum. The staff has decided in favor of the *Bulletin,* and plans for the timing of the Forum's first appearance and the consideration of other problems are being taken up now in consequence of that decision.

The Letters and Comments section of the JOURNAL will still be continued and will always be open to comments on the research reports appearing in the JOURNAL, discussions of the editorial policy, and even to more general views and interpretations dealing with the field of parapsychology.

A further device for supplementing editorial perspective is that of the symposium. In last December's number we discussed the advisability of holding such a symposium on the appraisal of the present stage of progress and of current developments in parapsychology. The proposal was made that the contributors be drawn from representative fields adjacent to parapsychology in order to bring to the discussion the advantages of differences in viewpoint. Again, reader approval and the further thought given to this proposal have led us to decide on the presentation of this symposium in an early issue of the JOURNAL.

Naturally, few readers would have an opportunity to know the

amount of work that goes into the whole editorial operation. The labors of the Managing Editor and the General Editors are to be taken for granted. What may not be known, however, is that every paper in the JOURNAL OF PARAPSYCHOLOGY that involves any statistical problems whatever is gone over carefully before publication for adequacy of evaluative methods by the Statistical Editors, Dr. J. A. Greenwood and Dr. T. N. E. Greville. This generous contribution to parapsychology deserves express acknowledgment. Recognition is due also to our Advisory Board. We have been receiving from its members on every occasion on which it has been invited the most capable and generous cooperation.

RORSCHACHS AND ESP SCORES OF PATIENTS SUFFERING FROM CEREBRAL CONCUSSION[1]

By GERTRUDE R. SCHMEIDLER

ABSTRACT: A short series of ESP card tests was given to 29 hospitalized patients. Eighteen were suffering from cerebral concussion, six had just recovered from it, and five had fractured bones but no head injury. The 18 patients with concussion had a significantly high total ESP score. The 11 other patients as a group scored slightly below chance average.

Ten of the 18 concussion patients were given Rorschach tests. The results of these tests taken together with the typical behavior of the concussion patients suggested to the experimenter that the high scores of the concussion group as a whole may be attributed, not to the results of the concussion per se, but to the passive, uncritical attitude which characterized these patients.—Ed.

THIS experiment is concerned with the ESP scores of hospitalized patients who were suffering from cranio-cerebral trauma, and who did not seem to have any focal injury to the brain.

PROCEDURE

The basic plan of the experiment was simple: it was to have patients who were not yet recovered from the effects of cerebral concussion take ESP tests and Rorschachs; to give the same tests to other patients hospitalized for a similar length of time because of accidents which did not involve brain injury; and to compare the data of the two groups with each other and with the norms for the nonhospitalized population. Unfortunately, the research was hampered by a practical difficulty; the crowded schedules of both the experimenter and the doctors in charge of the patients. As a result of the busy doctors' difficulty in finding time to diagnose the patients and to notify me while the effects of concussion were still present, and then of my difficulty in reaching the hospital at hours that would not interfere with hospital routine, the number of patients to be examined was only twenty-nine.

[1] Gratitude for their help in making this experiment possible is extended to Dr. David Arbuse, who arranged that the facilities of Morrisania Hospital be offered to me; to Dr. Gardner Murphy, who introduced me to Dr. Arbuse and planned the research with us; to Dr. Chodash and Dr. Sheckman, who were in constant touch with me as to which patients were suitable for testing; and to the nurses and attendants at Morrisania Hospital, who gave invaluable help with the practical arrangements for the tests.

ESP Material

The ESP cards which were the basis of the ESP part of the experiment differed from the usual material in two respects: the fact that the decks consisted of only ten cards, and the fact that the side of the ESP card on which the symbol was printed was painted in one of five colors (red, green, yellow, blue or brown). The subject was expected to try to guess the color, as well as the symbol on the card.

The target series was prepared by assigning two digits to each color, and two digits to each symbol. The experimenter read off the order of the colors from a randomized list of numbers and recorded it; did the same for the symbols; and then paired the first color with the first symbol, the second color with the second symbol, etc. A permanent written record was made for each series of ten such pairs; the corresponding painted ESP cards were piled in order; and both cards and written record were slipped into an opaque, unmarked container (a box or an envelope). A large number of these containers was available at all times; as a result the experimenter could not identify the contents of any of them.

It was decided before the experiment began that no distinction would be made between colors and symbols in scoring, since mathematically the odds for a successful response were the same for both. Thus the list of ten painted cards was scored as if it were a list of twenty ESP symbols.

The reason for modifying the customary procedure by this introduction of color and by shortening the deck was that normal subjects quickly became bored with a long series of guesses; and it was anticipated that concussion patients would quickly become tired. It was hoped that responding to a colored card bearing a symbol would be psychologically equivalent to a single response (just as it is hardly more difficult to read the word CAT in tachistoscopic exposure than to read the letter A) and thus that the patients would be hardly more fatigued by responding to both color and symbol than by responding to the symbol alone. Thus the number of responses given by the subject would be doubled with, it was hoped, minimum increase in discomfort to him.

One uncolored sample of each symbol was retained in the experi-

menter's kit, as were five unmarked cards (the reverse side of children's "trading cards"), each painted in a different one of the five colors used for the targets.

Administration

A nurse or attendant introduced the experimenter to the patient, and whenever possible screens were put around the patient's bed to reduce the distractions of the rest of the ward. The experimenter explained the procedure and arranged the ten sample cards where the subject could see them. This was comparatively easy where the patient was lying on his side or could turn his head; but in some cases where the patient was lying supine and almost motionless, the experimenter had to hold the cards above him. The patient was asked to indicate his "guess" about the first card and was permitted to do so by stating the color and symbol or by pointing to the appropriate samples before him. The experimenter recorded each guess immediately after it was made.

The opaque container holding the target was screened from the subject throughout the experiment. After the first pair of responses was recorded, the experimenter pulled out the first card and showed it to the subject. The subject then made his second pair of guesses, the experimenter recorded them and showed him the second card, and so on. The subjects were strongly urged to respond to ten cards, but some patients were unable or unwilling to complete this series.

After ten pairs of responses had been made, the subject's name was written on the permanent record of the target at which he had guessed. The Rorschach test was then given in the standard manner, except for minor changes necessitated by the cramped space or the patient's disability. In some cases, for example, the experimenter held the card and turned it as the patient requested, instead of putting it in the patient's hand.

Results and Discussion

Between March, 1950, when the experiment began and the end of June, 1950, when it terminated, 18 patients were tested who were diagnosed as suffering from cranio-cerebral trauma. Ten of these patients completed the Rorschach; one began but was unable to com-

plete it. Six additional patients were tested who were diagnosed as having had cranio-cerebral trauma but who were discharged from the hospital on the day of the test, and thus had presumably recovered from noticeable effects of the concussion. Five of these took the Rorschach; one refused to take it. Five patients were tested who had recently been hospitalized for fractures due to accidents but who did not have a head injury. The data will be described in terms of the questions that arose in the course of the research.

Question I. *Do patients suffering from cerebral concussion have unusual ESP scores?*

The answer to this question seems to be an unequivocal affirmative. Of the 18 cerebral concussion patients tested, 13 had scores above the average expected by chance, and only two had scores below "chance" (Table 1). These 13 made 341 guesses (equivalent to 13.64 runs on the basis of 25 guesses to a run) with a deviation of +22.8. Even with so few guesses, the data are significantly different from "chance" at the level of 2P = .002 for the cerebral concussion patients as a group. If we compare the 18 patients suffering from cerebral concussion with the 11 patients in the control groups, the

Table 1

General Results

	Number of Patients				Number of responses	Hits	Dev.	CR
	ESP scores above chance average	ESP scores at chance average	ESP scores below chance average	Total				
Total patients suffering from cerebral concussion	13	3	2	18	341	91	+22.8	3.08
Patients with recent fractures but no cerebral concussion	1	1	3	5	82	15	− 1.4	.63
Patients who had had concussion, tested on day of discharge	1	1	4	6	120	21	− 3	.68
Total patients not suffering from concussion at time of ESP test	2	2	7	11	202	36	− 4.4	.77

ratio of subjects above and below the expected chance average may be evaluated in a two-by-two table. It is necessary to use the method for obtaining the exact probability of either the observed or of a more extreme distribution, since the smallest cell expectation is not so large as required for the usual chi-square evaluation (3). By using the exact method, the probability for the difference between the two groups is found to be less than .003.

Question II. *Are the high ESP scores of the concussion patients to be attributed to the effects of concussion or to other factors that characterized the group, such as having recently had a serious accident, or being hospitalized?*

It was impossible to collect a large number of control cases from patients who had been recently hospitalized because of accidents which did not involve head injury. The second and third groups of Table 1, however, can be used to give a partial answer to our question. These patients, five of whom had recently been hospitalized for fractures, and six of whom were concussion patients who were still in bed but who were discharged on the day of the ESP test (and thus had presumably recovered from the concussion), had ESP scores that were slightly but not significantly below the chance level. The control data therefore imply strongly that neither hospitalization nor the shock of a recent severe accident will cause patients to give high ESP scores under the conditions of the experiment. We can therefore conclude, tentatively, that the high scores of the concussion patients can be attributed to the effects of the concussion.

Question III. *What other changes are characteristic of the concussion patients?*

Two types of material are available here: observations of the behavior and comments of the patients, and analysis of their Rorschach records. Because only ten of the group completed their Rorschachs, the former is probably the more revealing.

The most noticeable characteristic of the concussion patients was their inertia and their vagueness. They typically lay quiet in bed, not reading, talking, or moving. It was often hard for the experimenter to judge whether they were asleep or awake, even though the nurses would state confidently that they were awake, would speak to them, and would elicit an unstartled, quiet, mildly

cooperative response. They were relaxed or inert rather than tense, even when they complained of pain; movements and speech were slow; they were passively cooperative or passively, silently relaxed. Typically their facial expressions were relaxed, as in sleep; sometimes they smiled; sometimes tears came to their eyes; but they very seldom laughed or grimaced. As compared to patients suffering from fractures, they exerted themselves very little to help the experimenter arrange the materials, even when they were most cooperative.

All this is, of course, an impressionistic account, unquantitative, uncontrolled and summarized in spite of many exceptions. It is, however, consistent with descriptions given to the experimenter by the nurses, and with some comments in other published material. The observations would probably be easy to check, if further research along these lines should be performed.

The sparse Rorschach material that is available from the concussion patients does not deserve quantitative analysis, because no large group of records drawn from individuals of similar age, national background, education and socio-economic level is available for comparison. Three outstanding characteristics of the records may, however, be mentioned:

1. They were extraordinarily short. The range was from four to eighteen responses, with a median of nine. Records with nine responses are in the fifth percentile of the normal population (1).

2. The form level was unusually poor. Only 58 percent of the responses had good form; while the lowest figure quoted for the normal population is 70 percent good form responses, at the fifth percentile (1).

3. Although seven of the 10 subjects gave some color response initially, the color was integrated with an adequate form response (FC) by only two of the subjects; while six of the subjects gave responses where color was dominant over form (CF or C). The customary ratio, in the normal adult, is either to have more FC than CF or C responses or else, perhaps, to have approximately equal numbers (1, 2).

The implications of these three items are consistent with the behavioral picture described above. The small number of responses

and the poor form level indicate that the patients made little effort to respond and were inaccurate and uncritical in their responses to reality. The preponderance of color over form in the color responses indicates further that the patients responded freely and impulsively to impressions from the outer world. If this latter characteristic had been combined with a larger number of responses, it might have been taken to represent the forceful, egocentric behavior pattern that is likely to lead to selfish or even criminal behavior; in these patients, however, it would seem to indicate a feeble readiness to accept and to respond spontaneously to whatever stimuli come from the outer world, no matter how illogical those stimuli appear to be. This is an interesting point to note in connection with the marked ESP success of this group of subjects and lends itself readily to interpretation.

Question IV. *Do individual differences among the subjects confirm, contradict, or supplement the general trend of the data?*

It is of interest to examine the records of the five concussion cases whose ESP scores were at or below the chance level in order to check on whether they show other differences from the rest of the concussion cases. Since there are only five such subjects in this experiment, it may be worth while to examine them individually.

Concussion Patients with ESP Deviation at Zero

Mr. T. J. was described by the attending physician as a conversion hysteric. The physician stated that although the patient had actually suffered a concussion, he was exaggerating his disability, probably for the sake of the insurance.

Mrs. O. was a seventy-six-year-old woman, tested during the first month of the experiment. Later both doctors and nurses warned me against testing any of the older patients, because in their experience at this hospital, the older patients were so withdrawn as to be uncooperative. The record of this patient would support their contention, since, for example, one of her first comments to me was, "When you're old, there's nothing left for you." This was an unsolicited remark, not to be expected as a response to anything the experimenter had said, and was almost her only spontaneous statement.

Mr. T. had been injured five days before the ESP session. He complained of a dizzy headache, which he attributed to constipation. His ESP responses were made very quickly and gave the impression that he was impatient with the rules and procedure. After the tenth pair of responses, the experimenter told him that the series was ended. He then hurriedly made one additional pair of responses: a blue cross. This is noteworthy because the last card, which had just been shown to him, was a blue cross. At no other time had his response reproduced the previous card.

Concussion Patients with ESP Deviation below Chance

Mr. H. K., sixty-two years old, was hit by a taxi. His arm and leg were broken, he had bruises and stitches on his head, and bruises on his side. The ESP test was taken twelve days after his injury. He was eager, cheerful, and cooperative throughout the test and thanked me at the end of it. His deviation was −2.

Mr. H., sixty-five years old, moaned often and spoke of his pain frequently during the experiment. When the experimenter first came to his bed, he spoke about insurance and said, "I have no money with me." The experimenter explained that she had no connection with insurance; nevertheless a little later he spoke about insurance again. Still later, while in the course of making his seventh pair of ESP responses, he said, "I don't know what's in it for me."

Only one of the first 14 of Mr. H.'s responses had been correct; thus his deviation after the seventh card was −1.8. At this point the experimenter offered him ten cents if he was correct on either the color or the symbol of a card, and twenty-five cents if he was correct on both. He was completely correct on his ninth pair of responses, thereby earning twenty-five cents and bringing his total ESP deviation to −1.

While observations like these do not, of course, offer a sound basis for clearcut conclusions, they are often a profitable source of suggestions as to which possibilities to examine if later research on the same or a similar topic is to be performed. Tentative conclusions derived from them may therefore represent a useful step in inquiry. As hypotheses, subject to modification or to discard, we may accordingly say that *perhaps* Mr. T. J.'s low score reflected his lack of cooperation with the hospital staff; that Mrs. O.'s zero devi-

ation may have been due to her withdrawal, her unconcern with the world around her; and that Mr. T.'s quickness and his impatient manner marked such a difference from the typical concussion patient that psychologically he should not have been classed as a member of the group, whatever the neurological picture. For the two patients with ESP scores below chance, Mr. H. K. may already have recovered from the effects of the concussion, even though he was still suffering from his other injuries; and Mr. H., like Mr. T. J. and Mr. T., did not seem to be characterized by the usual passive inertness of concussion cases.

If these hypotheses are correct, they imply that it is not the concussion *per se* that changes neurological conditions so as to result in a high ESP score, but that it is rather the patient's willingness to drift with his illness which sets up a relaxed, receptive attitude and an uncritical willingness to respond to the outer world (with a minimum of exertion). He may therefore be more willing than the normal, alert, reality-centered adult of our culture to respond to the faint (and apparently illogical) perceptions which we call ESP.

Summary and Conclusions

Patients suffering from cerebral concussion had unusually high ESP scores, significant at the level of $2P = .002$. A small control group of hospitalized patients who had recently had severe accidents without cerebral concussion did not give high ESP scores. The behavior of the concussion patients was typically slow and relaxed, and showed a minimum of exertion. Their Rorschachs were characterized by few responses, poor form level and color dominant over form, implying (like their behavior) that they would exert themselves very little, and further that their general pattern of response was rather an acceptance of whatever stimuli came to them from the outer world than a critical sifting and analysis of their perceptions. A tentative conclusion, based on these observations and on analysis of the concussion patients with low ESP scores, is that the high ESP scores of the group taken as a whole may be attributed to a feeble, passive attitude which makes them refrain from the usual critical analysis of events that characterizes the adults of our culture, and to their willingness to accept impressions which come from the

outer world even if those impressions do not seem to be based on reality.

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THE PROBLEM OF PSI-MISSING[1]

By J. B. Rhine

ABSTRACT: Why is perfect success not the rule in psi performance? Dr. Rhine has been studying test series in the ESP and PK researches that show large negative deviations—series, that is, that fall below the scoring level expected from chance alone. In many such series the subject is to a significant extent missing the target he is consciously trying to hit. This effect is called psi-missing because it assumes the operation of psi in consistently making a wrong response.

The extensive array of the known psi-missing results is reviewed in this paper. The conceivable ways of explaining psi-missing are considered, and early in the paper a working hypothesis is suggested. This hypothesis is then tried out on the main evidence of psi-missing as it is surveyed.

It is suggested that psi-missing can best be explained as an unconscious but consistent adherence to an erroneous cognitive judgment; the subject sticks to a wrong way of responding but does not know he is doing it wrong since the psi process is wholly unconscious. Because the subject is unaware of his error, the more psi he exercises the more he misses the target. The further suggestion is made that certain strains, conflicts, and other disturbing influences in the subject's attitude are likely to cause this tendency to erroneous judgment. Among the conditions associated with psi-missing are certain personality states and traits such as skepticism about psi, a tendency to introversion, long delay in checking up on the test performance, and a number of other situations that tend to induce frustration and distort delicate judgments.

Finally, an effort is made to try to reconstruct the place of psi-missing in the operation of psi as a whole and suggestions are offered as to how psi-missing may be dealt with and controlled to a great extent in the researches of the future.—Ed.

I. Introduction to the Problem

The current of interest among students of parapsychology has for some time been turning toward the problem of increasing the degree of control over psi functions. This trend does not imply an abatement of interest in the *understanding* of these capacities; it is rather a stage in the natural development of research perspective. Once there is a certain amount of conviction reached as to the occurrence of psi phenomena, the question logically arises, if only from the analogy of sensorimotor functions: Why can they not be produced more easily and dependably? Why not, indeed, with perfect success?

[1] The studies reported here have been aided by a research grant from the Rockefeller Foundation. In addition to the help of the Journal advisory staff I wish to acknowledge the assistance of Drs. Gardner Murphy, Gertrude Schmeidler, S. G. Soal, and R. H. Thouless in reading and criticizing the paper, and the help of Dr. Betty M. Humphrey and Miss Elizabeth McMahan in assembling the material.

The Unconsciousness of Psi and the Problem of Psi-Missing

Recognition that psi is unreliable helped to draw attention to the characteristic fact that the subject is unaware of its occurrence. He has no conscious experience to enable him to judge whether or not his attempt to exercise psi capacity in any given case is successful. Otherwise only known successes would be registered by the subject as accepted psi responses, and to that extent adequate control would be achieved. In that case there would be few failures and much less need than now for statistical methods of evaluation.

It is true that in her analysis of spontaneous psi experiences Louisa E. Rhine (**39**) has found that a large portion of the cases collected do show experience of conscious conviction on the part of the subject. On the other hand, there is almost no reliable experience of conviction to date in the experimental tests of psi. As a result of this unconsciousness of the function, its effect is caught only by setting up a series of tests in which psi is encouraged to operate; accordingly, whatever effect is obtained is diluted by and spread through the various non-psi responses which the test procedure produces. The lack of perfect success in such test procedures, then, is fairly clearly a natural consequence of the subject's unconsciousness of whether in any given trial a psi effect or just a random response has taken place.

The unconscious nature of the psi process, however, is not the only difficulty the experimenter has to face. In addition to this uncontrolled dilution of the psi effect due to ignorance of when psi actually operates, there is a curious tendency toward reversal of the effect. In tests that allow for such effects, reliable evidence has in many instances been found of psi-missing, of the actual avoidance of the target by the subject in his responses. This target avoidance will in time of course bring the total score average significantly below mean chance expectation.[2] It has been found to do so in enough series to leave no doubt of the genuine occurrence of psi-missing (that is, of significant target missing requiring psi as an explanation).

It can easily be seen that this negative trend of deviation, com-

[2] The case for psi-missing in a given series is of course not based on a mere selection of that portion of the series (or those series) which happen by chance to give negative deviations from expectation. The same standards are applied as are used on the more common positive deviations in psi-hitting. In fact it has become standard practice to compute probabilities on the assumption that either a positive or a negative deviation may be considered as a possible experimental result.

bined with the scatter of psi hits among a large number of non-psi responses (that is, chance misses), might well prove to be a crippling handicap to the experimenter. It is especially bad if, at one part of the experiment (let us say in one-half of the subjects taking part or in a large chronological division of the series), something develops to evoke psi-missing and that effect cancels all the positive deviation accumulated in the other half.

Suggestions as to the Way Psi-Missing Works

In the light of what is known of the unconsciousness of psi, it is not surprising that this psi-missing effect has been found in many experimental series. Nor is it difficult to conceive of ways in which it can occur. To consider one of these ways: it may be supposed that the subject has some unconscious resistance to the purpose of the experiment or to his participation in it. Negative reactions of unconscious nature are by no means unfamiliar in psychology. Since psi operates on the unconscious level of mental life, one could expect that in cases of negative motivation there would be an avoidance of the target and in consequence an accumulation of a negative deviation.

It is not necessary, however, to assume that the subject has a negative attitude in order to account for psi-missing. Such missing could be a kind of persistent psi illusion or error, a self-deceptive psi response by the subject. Any persistent cognitive error could give systematic missing in the tests, just as a cognitive visual error can mislead a rifleman to fire regularly on a wrong target on the rifle range and avoid the right one. Any consistently wrong procedure that would lead the ESP subject to the habit of rejecting the symbol brought to mind by the true psi process would automatically favor psi-missing.

Suppose, for example, that the right way for a subject is to take the first symbol that comes to mind; then, if the subject is made to go more slowly, he may unthinkingly suppress the first tentative image and regularly take another one, a different one, which would be, of course, the wrong one. Still another example of a way psi-missing could operate is that called "consistent missing" by Cadoret and Pratt (**5**). They found that some subjects systematically called certain symbols incorrectly, misnaming them in the same way (for

example, calling a star a cross). This was a kind of ESP illusion and by itself would be a kind of psi-missing effect. Or again, merely displacing ESP to the next card ahead and undercalling doubles (that is, not repeating) would give a negative deviation; in fact, it is known to have occurred.

These two general types of explanation, negative motivation and systematic cognitive error, both operating unconsciously, are submitted as applying to most of the evidence of psi-missing so far discovered.

Importance of the Problem

Before going into a consideration of the actual evidence, however, some readers may welcome a further word as to why this survey is needed. It is not enough to say that a great amount of evidence of psi capacity has probably been lost through the operation within the total experiment of strong counteracting currents in the form of this psi-missing. (It is now well known that later breakdowns and analyses have in many instances revealed substantial evidence of psi when the two tendencies, the positive and the negative, were separated by means of proper statistical procedures.) Now that evidence of psi in itself need no longer be the major objective of a research, this psi-missing should be recognized as a great barrier to progress in current research as well, research directed toward the understanding and control of psi capacity. It is the growing realization of this retarding role that psi-missing has been playing all the way along in the research that has led to this paper.

Indeed, a great portion of the comparative researches done in the last decade on conditions and factors related to psi performance are not interpretable unless we isolate and recognize a psi-missing operation as such and distinguish it from the measure of psi itself. In the study of psychological states or processes associated with success and failure in psi tests many significant separations of high- and low-scoring groups have been found, and these are important steps on the way to the understanding of the nature of psi.

It makes a great difference, however, as to where the separation line lies. If both groups are scoring above mean chance expectation it is usually assumed that a difference, if significant, is a difference in the amount of ESP ability operating in the two groups.[3] If,

[3] It will be well to keep in mind, however, that there are other (even though less likely) possibilities.

however, one group produces as large a deviation on the negative side of the mean as the other produces on the positive side, one cannot safely draw conclusions from the score comparisons regarding the amount of ESP capacity demonstrated by the two groups. It can only be said that the one has produced the positive deviation expected and the other has, for reasons unknown, produced a negative deviation, perhaps one that is as significant as the other. If ESP is responsible for both deviations, and they are both equally large proportionately, then as much perceptual ability is required on the whole for the one deviation as for the other.

It is often hard to tell whether, in a given research, psi-missing is an important element or not. Certainly we cannot suppose that when it is active its effects will necessarily be so grouped and so evident that they will inevitably be discovered. Instances will be given in this report of experiments in which the run (or set or series) begins with a positive deviation and ends with an equally marked negative deviation. In a series producing such a distribution on both sides of the mean, the deviations would cancel and the total would be very close to zero. If, then, the psi-missing effect that depressed the scoring rate at the end of the run was not discovered in the analyses, the experimenter would naturally conclude that his experiment had given only chance results. Furthermore, if he compared the results of this series with those of another in which no negative deviation occurred at the end of the run, he might well find a significant difference between the two and conclude that he had a reliable difference in the amount of ESP registered under the two sets of conditions. Yet such a conclusion would be misleading. The actual determinants would be obscured, and the confusing factor would be psi-missing.

The operation of this uncontrolled factor has probably been one of the most hampering obstacles encountered in psi research. This estimate will perhaps appear more reasonable as the extent of psi-missing and its neutralizing of positive results is appreciated. Such appreciation is itself limited naturally to what has been found of the evidence of such missing, and no one can say how much remains still undiscovered in the research files and reports, in results labelled "insignificant." It is the aim of the present survey, however, to bring to the attention of the research worker the main body of evi-

dence of score reversal that has been found in the work reported. With such a warning he may be less likely to have his efforts defeated by the undetected operation of psi-missing in his own research.

One more point should be made. Those who stand far back from the pit where the research findings are dug up often speak unappreciatively of the routine card-calling and dice-throwing tests of psi capacity. Yet it is only in such tests as these that this leakage represented in psi-missing could ever have been caught and studied as a factor subversive to the experimental objective. In a test that did not allow for a close measure of the results to be expected from chance alone, it would have been impossible to discover these effects that are below the chance average; yet psi-missing would presumably have operated nevertheless, even though in complete obscurity; as it is—and if, of course, they can be caught and isolated—the negative deviations can be as well studied as the positive.

A word about procedure: After a review of the earlier examples of psi-missing encountered, I will consider explanatory hypotheses, take a tentative concept to work with, and proceed then to a fuller review of the main types of conditions under which psi-missing occurs. The aim will be to try out the working hypothesis as an explanation of what is known, as well as to provide a fair survey of the extent to which psi-missing has occurred.

II. Psi-Missing and Declines in Scoring

Some Early Examples in ESP Tests

The first case of significant target avoidance to be noted in the psi literature was that of Estabrooks' Series No. 4 in the ESP experiments carried out at Harvard in 1926 (**9**). His first three series gave results above the chance mean (five hits are expected on suit in the run of 20 playing cards used). In the 32 runs of Series No. 4 the average dropped to 4.06 hits per run (with 5.00 expected). Such a result gives a negative deviation that is statistically significant.[4]

[4] It increases the value of such total figures involving group performance to know the degree of consistency of the effect among the group membership. For this survey, however, the consistency figures are not included: For one reason, there is so much to survey that economy of space and effort is important, and the paper is overlong as it is. Also, in most of the personality studies to be reviewed the consistency evaluations have been made and have been found to be significant. There is no undue risk, then, especially at this stage of the study of psi-missing, in leaving the details on the amount of consistency to a more refined stage of inquiry—and to the original research reports concerned.

The series was different from the other three, however, in the way it was conducted. The subjects had taken part in an earlier series; they were requested to do this series as an additional task, and some or all may have been constrained to do so only out of courtesy. There is also another point of difference: the distance between the sender and receiver was increased. This change may have made the task seem harder to the subjects. Even so, the hits in the twenty-trial run for this series as a whole averaged above the mean at the start of the run and did not drop below until the fourth trial; there was then a marked decline throughout the rest of the run.

In the earliest experiments at the Parapsychology Laboratory at Duke this tendency to decline below mean chance expectation was noted also, especially in long runs. H. F. Frick (**32,** p. 54) in an ESP experiment with playing cards, made nine long runs of 100 trials each (trying to identify suits) and produced one of these marked declines. Taking the composite run for the whole series in segments of 20 trials each, the deviations are as follows: +13, +5, +3, −7, −9. During the same period another ESP experiment was made with A. J. Linzmayer (**32,** p. 66) as subject in which six long runs of 50 trials each were made with the ESP cards (enclosed in sealed envelopes). The run breakdown in five segments gives the following for the total series: +7, +1, +3, −3, −8. In this, as with Frick's series, there is sufficient statistical significance (P is less than .01) to be attached to the differences between the deviations of the first two and the last two segments of the runs to justify serious consideration of what may be going on, and to regard it as extra-chance in character. The negative deviations are about as large as the positive ones.[5]

In these experiments by Frick and Linzmayer there is what looks like the same effect, a decline in the run, as that found by Estabrooks in his Series No. 4. Estabrooks himself had noted, as had others before him, a *general tendency* of the subject in psi tests to decline in scoring rate with the succession of trials in the run. Richet (**40**) had stressed that this was an effect of having too-long runs. Jephson (**21**) observed such a falling-off even in very short runs of only five trials, and still others have observed such results

[5] While this result is not as conclusive of the operation of a psi-missing factor as if there were a negative deviation significant in its own right, it is regarded as of sufficient interest for the present discussion.

since then. Such a decline in scoring rate may be caused by a falling-off of the amount of ESP itself or it can be due to the obscuring effect of a contrary tendency toward psi-missing. The question arises, then, as to whether Estabrooks' negative Series No. 4 is merely an extreme case of the decline factor that worked in his other series. These other series, too, gave significant differences between the first and second halves of the run, even though both were on the positive side of the mean.

No one thought even to ask the question at the time and to analyze the data for a possible answer. Today, as the problem is being examined, it seems most efficient to isolate first what can be found out about psi-missing in general. Then, with our knowledge of that phenomenon somewhat clarified, it should be easier to undertake to look into the decline-producing factors as a distinct project. In that way, there will be some hope of disentangling the different factors and their effects which are at present so easily confused.

Low-Aim Tests and the Concept of Partial Perception

When interest first arose in the Parapsychology Laboratory as to what these negative deviations meant, one of the first steps taken was to try consciously for low scores by attempting to call each target symbol wrong. At a stage when the subject Hubert Pearce was averaging approximately eight hits per 25 trials in normal BT tests (**32,** p. 80), he obtained, in low-aim tests, an average score of 1.81 in 11 runs (with 5.00 expected). Stuart (**32,** p. 70), in 52 low-aim runs (of 25 trials), obtained an average of 3.50 hits, while in the high-aim trials made on the same cards his average number of hits per run was 6.56. The two deviations (from 5.00) were thus approximately equal. Pegram (**28**), in a long series of high-aim ESP tests (like Stuart's, self-testing and with significant results), averaged 5.30 in 5,930 runs, while she averaged 4.35 in 1,468 runs for low aim. A deviation of +.30 per run for high aim is of course much smaller than the −.65 per run of the low aim. While the two conditions were not always entirely comparable, the comparisons may be taken as justifying further steps in the study of factors producing negative deviations.

The low-aim experiments showed, at least, that psi capacity capable of producing positive deviations in test conditions would

produce negative deviations of comparable magnitude if the subject attempted to *miss* as many targets as possible, *even though the chances of making a hit or a miss were unequal* (1/5 and 4/5). This fact was far from an obvious one and not a logically necessary one. For if ESP were an all-or-none operation there would be expected on the ESP card tests (with five possible targets) four times as great a deviation in high-aim tests as in low-aim, given the same amount of ESP capacity. This may be seen best by illustration. Let us suppose a subject knows five cards so that he can perceive them unerringly and then on the remaining 20 he gets four hits by chance. On this assumption he would expect to average nine hits per 25, or four above normal expectation from chance. Now if he tried to *miss* them all he would expect to get four hits by chance on the 20 he did not know, or a negative deviation of only one hit per run. Why should the same amount of knowledge give this large difference in score deviations? The difference is that the subject's *perfect* knowledge of the five cards which helps him to make five hits on the positive side is largely wasted in his attempting to avoid hitting the symbols (because it can help him only to make five misses). He does not need to know exactly what the five cards *are* to say what they are *not*, and accordingly his more complete information on the five cards cannot register effectively in target avoidance.

As a result of the findings from the low-aim comparisons as well as from the general tendency (as will be seen in this review) of the psi-missing effect to produce negative deviations equal to positive ones produced under comparable circumstances when psi-missing was not operating, the conclusion was reached (33) that psi does not operate on an all-or-none basis. Rather, in target-avoidance it seems definite that only a partial or limited perception is exercised. It is likely that such a partial perception is operating in much of the target-hitting too, with only occasional trials adequate for scoring a success. It is not, of course, peculiar to extrasensory modes of perception, but is probably general to all sensory perception.

Psi-Missing and Run Declines in PK Tests

The psi-missing effect is not limited to the ESP types of psi process. As a matter of fact, a number of examples of psi-missing in PK experiments will be mentioned as the review proceeds. In one

of the earlier PK researches a decline was encountered within the run which produced a striking negative deviation at the lower end of the run, just as occurred in the ESP researches mentioned above. In this series, reported by Rhine, Humphrey, and Averill (**38**) under the title "An Exploratory Experiment on the Effects of Caffeine upon Performance in PK Tests," the columns of results were made up of only five throws each, but each throw consisted of the release of 96 dice at once. Each run, then, was a relatively long one in time required and number of dice handled. The deviations for the five throws of the column in the 7,200 single die-readings for the pre-caffeine series are as follows: +25, +38, +11, −17, −24. There is a significant difference between the first two throws of the column and the last two. The most interesting point in the series, however, is the fact that in the post-caffeine series, 9,120 die-readings, the decline, though still marked, did not carry over to a negative deviation. The following are the deviations for the five throws of the column for the entire post-caffeine series: +53, +56, +24, +32, +18. The CR of the difference between the first two and the last two throws is not significant. Whatever was responsible for the negative deviation of the pre-caffeine series was either overcome or obscured in the post-caffeine, though there was still a drop of considerable magnitude from the scoring of the first two to that of the last two throws in the total post-caffeine series. This decline cannot be identified as the result of psi-missing, but it could well have been due to that.

Other Position Effects and Psi-Missing

There have been many chronological declines in which there were significant differences between the first and the last divisions in the series. In some of these declines, too, the deviations have swung over to the negative side as far as or farther than the positive deviation with which the series started. For example, Casper, in his article "A Further Study of the Relation of Attitude to Success in ESP Scoring" (**6**), reports a deviation of +50 for the first half of his 584-run series and −70 for the second half. The difference is significant.

A decline of scoring rate within the test session was reported by McMahan (**24**) in her PK experiment based on disc-throwing. Her

subjects, children and adolescents, gave a deviation of +113 on the first turns of the session (337.5 runs; CR = 2.75) and one of −88 for the second turns (337.5 runs; CR = 2.14). The difference gives a CR of 3.45.

Thus far the topic has been declines, declines in which there is a negative deviation at the end roughly equal to or greater than the positive deviation on the first half. There are, however, in a very few instances negative beginnings in series that have significantly positive endings. In the pooled totals of all the Smith series, Humphrey (**18**) found an incline in scoring rate. The subjects who (in ESP tests based on drawings) made compressive types of drawings (that is, those who made poor use of available space, etc.) showed an upturn of scoring rate on the two-run page of clairvoyance tests using ESP cards. There was a very negative deviation on the first run, significant by itself, and a positive one on the second run. The difference, too, is highly significant. Similarly, in the PK studies there is a markedly negative beginning in the McDougall One-Die Series reported by Rhine and Humphrey (**37**). The quarter-page distribution for the entire 230 runs shows a deviation for the upper-left quarter of −34; for the lower right, of +31. The difference between the two quarters is also significant.

A negative deviation has been known to occur chronologically in the first instead of the last part of the series, though the reverse is much more often the case. In the Schwartz PK experiments reported by Rhine (**34**) the first third of the series representing one of the three conditions of the experiment produced a significantly negative deviation (144 runs, −60; CR = 2.74). The other two sections gave score totals above expectation.

Although psi-missing is on the whole more likely to come in at the end of the series or the page or the run than it is at the beginning, it is important to keep both types of situation in mind since they can hardly be ascribed entirely to the same psychological factor. What these factors are and some of the other factors necessarily ignored in the present discussion will be the subject matter of later articles.

III. Discussion of Hypotheses

It is worth pausing at this point to consider how best to explain such localized negative deviations as that produced by the decline

of scoring in the run or series to a point below the mean. These psi-missing results occurring within the same set of conditions that normally produce positive results represent a seemingly simpler problem than some of the other series to be reviewed; that is, the factor responsible is to be sought in the subject's altered response to the unchanged external situation.

What causes this alteration of response? Which of the two hypotheses under consideration, negative motivation or a systematic cognitive error, is the more plausible explanation? It is difficult to suppose that unconscious negativism occurs during the second half of the run and is lost again when the next run begins. Likewise, it is not easy to suppose that Casper's subjects in the first half of his series wanted to score above the chance average, but that those in the second half wanted even more strongly to score on the negative side. When the beginning is negative as in the first Schwartz series and the first quarter-page in the McDougall One-Die Series, the initial psi-missing effect seems even less like a volitionally produced result than an unconscious cognitive fumbling of some kind involving the psi process. In other words, it looks more like psi error than negative motivation.

The error hypothesis is plausible. When the subject begins with a normal operation of the psi process and continues through a series of trials in rapid succession, there are accompanying psychological operations that consciously and unconsciously complicate the succession of judgments. We need only suppose that the subject unconsciously *alters his mode of psi functioning* so that the delicate decision which usually gives him his success is now unconsciously passed over in favor of an alternative mode that, without his knowing it, fails him. Perhaps the very act of trying too hard in the wrong way (easy to do since psi *is* unconscious) leads him to pass over the operation essential to success. As explained earlier this would automatically tend to bring about rejection of the target symbol and therefore the production of a negative deviation.

But to reject the idea of a primarily negative conation in these psi-missing cases does not require the dismissal of all motivation. Motivation merely becomes secondary. Schwartz likely began his dice-throwing series with a certain amount of intellectual conflict, and in spite of strongly positive conscious interest, there may have

been an unconscious skepticism that could have influenced the effort. The same can be supposed for the McDougall One-Die Series, though it is less likely to apply since this would have been necessary at the beginning of the run and would have needed to be a recurrent state of mind. In the case of the Humphrey report on the Smith series, the "compressive" subjects might very well have begun with a restraint that involved self-doubt of psi.

We shall see that most of the evidence of psi-missing lends itself to the generalization that some secondary conative disturbance would seem to be involved. If this is taken as plausible the cognitive error hypothesis (the view that the subject just unintentionally pushes himself "off the track" and blindly but persistently avoids the target that he somehow identifies) will appear more understandable. The motivation in this cognitive error hypothesis, then, need involve no negativism, either conscious or unconscious; but like all perceptual functions psi-missing must have its motivational basis. Various conative factors will be seen to be involved, quite apart from anything like negativism.

IV. Examples in Which Unconscious Negativism Is a Possibility

Before going on to the hypothesis of cognitive error as the more acceptable explanation for most of the evidence of psi-missing, however, it would be well to look first at some of the examples that come nearest to being interpretable in terms of negatively motivated missing. Just how conscious or unconscious the motivation may be in these instances can only be inferred and need not be determined, though it is probably mostly unconscious.

In this connection one thinks first of the well-known differentiation of Schmeidler (**44, 45, 46, 48**) in her ratings of "sheep" and "goats" in ESP tests. Her goats, the subjects who did not think the ESP hypothesis acceptable, or at any rate did not themselves expect to score successfully, in general gave a negative deviation over a large number of series, while the sheep generally speaking gave a positive one. The differences for the combined series to date would be highly significant. However, we cannot and need not say these goats did not either consciously or unconsciously want to make hits in test responses, though it would be reasonable to suspect that some of them did not. We need only suppose that there was some attitude

(most likely a conflict of attitudes) that caused the delicate, unconsciously guided psi function to alter its normal effect. If the goats had been merely indifferent and unmotivated they would have given only chance results; and certainly Schmeidler's goats scored at extra-chance levels. As far as we know, there was the same ability, presumably the same effort, as the sheep indicated, but it was misdirected away from the targets.

Adams's report (1) offers the suggestive case of a "goat," in this case a psychiatrist, who was skeptical about ESP but willing to undergo the testing. He went through 50 runs as a subject in the normal state, presumably trying consciously to hit the target symbols, and produced a deviation of −13; then in 50 runs made in connection with carbon dioxide therapy treatments taken for research purposes he got −41. The 100 runs as a whole produced a significant negative deviation. The 50 under carbon dioxide are significant by themselves. The subject's conscious effort to get hits *may* have been only superficial. We cannot say.

In the Soal and Goldney research the subject, B.S., gave a negative deviation on the regular target in the same series in which he produced large positive deviations in displacement hits (that is, on adjacent symbols in the target series). Soal (**53**) interpreted this as a volitionally directed response on the part of the subject, who, he assumed, was trying to get displacement but not to get target hits. Accordingly, when he got a displacement hit forward, he needed to call a different symbol on the next trial to avoid making a hit. This was done to a degree that resulted in a highly significant negative deviation. Pratt (**29**), however, found that this target-missing tendency was not limited to the cases in which forward displacement occurred. Accordingly Soal's interpretation is not borne out. Likewise, in Soal's experiments with Mrs. Stewart (**54**), the latter made a highly significant negative deviation in the first trial of a pair of successive targets of the same type, the first of a double. Here, too, Soal offered a plausible theory for the motivation of the subject. In contrast to B.S.'s attitude, Mrs. Stewart did *not* want precognitive displacement and to avoid getting it she had to avoid making hits on the first of a double. More recent analyses (still unpublished) by Pratt and Soal, however, show that this avoidance of forward displacement is not a general practice of Mrs. Stewart, but

is confined to cases of doubles; hence Soal's suggested explanation of negatively motivated avoidance of the target in this series will not do.

Cadoret (**4**) obtained a series of results from a self-testing ESP experiment in which he tried a variety of techniques for keeping the test situation interesting. His first use of a novel technique gave positive results and thereafter gave a negative deviation. These negative deviations were significantly different from the positive deviations given on the first usage, and are independently significant. It appears likely that the subject unconsciously changed his motivation when he observed the effect of the loss of novelty. But this too is only a conjecture.

Woodruff (**58**), in a study of ESP under various physiological conditions, found that subjects confined to a chamber designed to control oxygen, temperature, and other conditions of the environment reacted with boredom to the confinement and restraint, and that they produced a significant deviation of −50 on 70 runs in contrast to a deviation of +41 on the 85 control runs made before entrance into the oxygen chamber. The suggestion, frankly hypothetical, would be that in the Woodruff tests, an experience of irritation (probably unconscious) may have developed in the subjects which resulted in the unconscious rejection of the target. In the test series of this section, it is not implausible to consider unconscious negative attitudes as responsible for the psi-missing. It must be noted, however, that there is no very definite and clear case for the hypothesis of unconscious negative motivation even in this group of studies where it should be most expected to be found.

V. Psi-Missing and the Effect of Contrasting Conditions

For most of the evidence of psi-missing, however, there is not as plausible an explanation to be offered as in the examples just reviewed. While no confident attitude could be taken in the matter, all the indications favor the hypothesis of systematic psi error resulting from the subject's reaction to the test situation. To illustrate: it has been observed from time to time by psi experimenters that when two test conditions or procedures are being compared in the same experiment the response of the subject to the two is likely to be more adverse to the less preferred than would be the case if it

were taken alone. Something more than mere drainage of interest is involved; indeed, it appears that some negative reaction to the less favored condition occurs.

ESP Tests Involving Two Conditions

This accentuation of differences may be illustrated in a report by Stuart, Humphrey, Smith, and McMahan, (**56**) in which two ESP test procedures were employed, one using ESP cards and the other drawings. In the 63 subjects who participated, approximately half should have given deviations on the same side of the mean in the two procedures and half should have differed. There were actually 42 who differed and 21 who scored on the same side of the chance average in the two methods. This suggests that a good share of the 42 who differed went to the negative side of the mean as a reaction to the comparison of the two procedures.

In a recent ESP experiment by Hallett (**11**), a group of subjects were given a total of 52 runs in which a double task was involved. The subject was asked not only to identify one of five symbols but also to place it correctly in one of five positions. The position aspect of the test gave a significant positive deviation; the symbols gave a significant negative one. Now the position test was a new one for most of the subjects and might be supposed to have the advantage of novelty. But why did the symbols draw a psi-missing response? Their unfavorable comparison with the more novel feature of position in the tests is suggested as a factor in the explanation.

Thouless, in his "Comparative Study of Performance in Three Psi Tests" (**57**), obtained a significantly negative deviation on one of the three methods used. The three tasks were carried out at the same session, one involving ordinary DT and the other two, precognition. To his surprise his scoring was negative on DT and positive on the two precognition procedures. Precognition was new and challenging; hence the positive scoring rate on precognition tests. But why did he score so negatively on DT? Possibly because it was the older and more familiar; but it probably was the adverse effect of doing the DT tests in a comparative experiment that led to the psi-missing results.

Under the title "ESP Test in Light and Darkness" Bevan (**3**) reported a deviation of −24 for 48 runs in the dark and +26 for 48

in the light; the difference is marginally significant. There was nothing to break the absolute darkness in the tests made under that condition, and the subject was alone in a small soundproof, light-proof room throughout the run. But it may be suggested that the negative deviation was probably due, not to the condition as such, but to the contrast given by the association of it with the tests in the light.

PK Tests Involving Contrasting Conditions

The effects of comparisons of light and darkness are not, however, confined to ESP; in a dice experiment Gibson and Rhine (**10**) reported a significantly negative deviation obtained in darkness with a subject who had generally produced a highly positive deviation under ordinary conditions. However, in one session this subject made 18 runs in darkness and produced a deviation of −24; in red light she gave a positive deviation of +6 on the same number, and in white light a deviation of −1. The series in darkness itself began with positive scoring. It is not very clear whether the negative reaction was in contrast to the other lighting conditions or to something else in the particular situation in this session. It was more tedious and slow to count the hits and make the records with a flashlight. What strains and distractions resulted from the darkness to distort the PK process can only be conjectured; but it seems most probable that there was something of a negative effect resulting from the comparison itself.

Quite the opposite result was obtained from a comparison of light and darkness in a PK experiment by McMahan (**25**) in which the objects used were discs. In this case the tests in darkness yielded a positive deviation and those in light, a negative deviation. In this experiment, however, there was a group of subjects always present, giving a social antidote to the isolating effect of darkness. The experimenter even found the subjects, who were children and adolescents, interested in the novel experience of experimenting in the dark. Also, the darkness was no great inconvenience; the discs were marked with luminous paint and since a rotating cage was used and they were easy to count with a flashlight, it was not necessary to pick up the discs. Two hundred and twenty runs in the light gave a deviation of −61, while the same number in the dark gave +54. The difference is significant.

In his PK work reported under the title "The Effect of PK on the Placement of Falling Objects" Cox (7) reports on a comparison of two aspects of a test made on the falling of dice—a test in which the subject attempted to influence the dice in two ways. In one series the primary task of the subject was to make the dice fall on squares designated by one of the numbers from one to six. The secondary objective in this series was to make the corresponding face on the dice turn up. In another series the primary task was to try to make the designated face on the dice turn up as the dice fell, the secondary task being to influence the dice to fall upon the designated squares. Whichever task was secondary, however, the deviation was negative on this aspect for all three series. The total of 1,632 runs for the primary test gave a deviation of +100, while the same number on the secondary test gave −426. The difference is highly significant. It is definitely suggested that the comparison introduces a strong psi-missing influence on the secondary features of the tests.

If there is any adverse conation at work in producing a negative deviation on the less preferable of two conditions that are being compared, it is probably very mild. It is not likely that in many if indeed any instances the subject would have admitted to any lack of desire to score positively on the less favored conditions or methods. We cannot, of course, prove that there is not some subtle, unconscious sabotaging going on in these delicate psi operations to produce psi-missing, but if so it has yet to be discovered. Even weak motivation, however, and motivation that is in no degree negative, may produce a slight shift of attention, some relaxation of effort, or an ever-so-subtle choice of an alternative judgment, and the operation of ESP and PK may be so easily misguided that no vigorous conational change is needed. As already stated, any systematic distortion of the device that normally gives positive deviations in psi tests may well produce as much of a psi-missing deviation as the subject would ordinarily be capable of achieving at psi-hitting if he were undisturbed. There is, therefore, no need to suppose any *negative* impulse at work in these comparative tests to explain the negative deviations produced.

VI. Cases of Psi-Missing Which May Be the Result of Conflict

There are factors that produce psi-missing effects that are still more obscure and involved than any thus far considered. It is now known that the personality of the experimenter or, it might better be said, the interpersonal relationship between the experimenter and the subject has something to do with scoring success; this relationship is suspected even of sometimes producing negative deviations.

Experimenter's Personality and Attitude

In a series of psi tests with children made by McMahan and S. L. Rhine (**26**) a marked difference was found between the results of the two experimenters. The one obtained a significant negative deviation from her tests; the other obtained a positive deviation that was not significant. They point out that the experimenter who obtained the psi-missing effect was new to the experiment and suggest that in the intensity of her conscientious concern to carry it out just right she may have put the child subjects under a greater degree of tension than the other experimenter. Sharp and Clark (**49**) report in "Group Tests for Extrasensory Perception" that on one occasion an experimenter who was definitely skeptical about ESP was given charge of the experiments. The 79 runs he supervised gave a significantly negative deviation whereas the work of the other experimenters taken as a whole was positive. The authors explained that the skeptical assistant showed little interest in the test and was given to distracting mannerisms.

Conflict from "Stage Fright"

Another delicate factor that may induce psi-missing is the conflict that should be expected in a subject called upon to demonstrate psi capacity before a scientific visitor or committee. In a few cases psi-missing has been associated with such conditions. One example of this is given in my report (**35**) of tests made on two dowsers who were asked to use the dowsing rod to try to tell whether the water in an underground pipe was flowing or still. The tests were made to find out whether a method could be developed to appraise the claims of dowsing. The dowsers should be expected to have been under some strain from the attention concentrated on them. Their results were significantly negative.

This depression of scoring, possibly due to stage fright, recalls the story related orally by Dr. Thornton C. Fry[6] to a group of fellow-mathematicians visiting the Parapsychology Laboratory in 1936. An electrical apparatus was claimed to possess diagnostic value in medicine when used by an operator who possessed suitable hyperphysical qualifications. To test these claims, the practitioner was given the task of distinguishing whether a specimen put in the machine consisted of blood or water. His score was precisely one hundred percent wrong. Because the process was a rather lengthy and tiresome one, the number of specimens which the practitioner would consent to analyze was limited to ten, and hence the result was not as conclusive as could have been desired. However, as Dr. Fry remarked, the result was the least probable and therefore the most significant of all those possible within the limits of the experiment, and entirely consistent with the hypothesis of temporary reversal of the ability which the practitioner had claimed. The man was very likely under great strain because he was up before an investigating committee that could expose him as a charlatan if he failed. It seems possible that he was using psi capacity in interpreting whatever indications his device produced, and when put on the spot, the induced conflict threw him off the track.

Intellectual Conflict in the Subject

There are several instances of psi-missing in the research literature which look like the result of intellectual conflict. In one of these cases a group of adults here at the Parapsychology Laboratory were confronted with a test of precognition. Certainly on rational grounds precognition is bound to appear impossible. Such a test is likely to induce a state of frustration. At any rate, in the adult section of the precognition experiment that I reported under the title "Experiments Bearing Upon the Precognition Hypothesis. III. Mechanically Selected Cards" (31) the 1,108 runs produced a deviation of −239, which is a quite significant result. A children's series conducted at the same time gave a positive deviation of comparable average magnitude. The children had no intellectual conflict; for them it was just a game. The members of the adult group were up

[6] The story also appears in a slightly disguised form on p. 41 of his book, *Probability and Its Engineering Uses* (New York: Van Nostrand, 1928).

against something in which it probably seemed to most of them impossible that they could succeed.

The introduction of rewards in this experiment accentuated the psi effect, and larger deviations for both groups resulted. In the light of this result, it does not look as if negative motivation could account for results in which the deviation and its significance were increased by giving rewards. Rather, it looks as if unconsciously the subjects, affected by conflict as they probably were, fell into an erroneous way of trying to express ESP and that this mistaken procedure was merely increased by the motivational support of the rewards. Again it seems likely that while psi-missing owes something to the intellectual conflict of the subject, the effect of such motivation is indirect.

Frustration Resulting from the Use of Enclosed Cards, Delayed Checking, Etc.

The Chutes Series of Humphrey and Pratt (**20**) offers a strong case of psi-missing. In this series, 490 runs yielded a quite significant deviation, −165. The test situation looks like one that would produce frustration. The subject was seated alone outside the research cubicle and was given a batch of cards each of which was enclosed in a sealed opaque envelope. He was asked to distribute these into five chutes so that the cards would match the key symbols marked on the chutes. To some extent the fact that the cards were enclosed, the necessary delay which occurred in scoring, the isolation of the subject from the company of the experimenters (who could not even be seen) may account in part for the target avoidance which began even with the first run and increased as the subject continued. He was told what his scores were but this information lagged at least one run behind. It seems likely that a certain isolation and mild frustration must have characterized this test which turned out to be so consistently negative.

There is something of a clue in the fact that, presumably because of the delay in checking involved, enclosed cards have in general tended to lead to psi-missing unless the calls were checked promptly at the end of the run and the subjects were told their scores. There are rare exceptions, as are likely to be found in any generalization about people, but a number of research examples can be assembled to illustrate what appears to be a rule.

In the earliest experiments with enclosed cards many of these were mailed out to subjects to be used in tests and returned. It was soon observed that those subjects who had to work on cards that had to be mailed back before any information regarding success could be obtained usually went below mean chance expectation. When, in 1938, the enclosed card work (**30**) was separated into that which was accompanied by long delay in checking and that which was checked immediately, the difference was marked. While the immediate check work was positive, the tests with long delay gave evidence of psi-missing to the extent of a significantly negative deviation. Russell and Rhine (**41**) obtained a similar result with the compartment box tests with subject Pope Hill, Jr. In the 534 runs in which the checking of the results had to be long delayed there was a deviation of −129, which was significant.

Less striking but still suggestive is the amount of psi-missing shown by the negative deviations of the correspondence tests summarized in the appendix to my precognition article already mentioned. For the total of 14,542 runs there was a deviation of −432. The CR is only suggestive (1.79); but by way of contrast, laboratory series of tests made during the period totalled 20,268 runs, with a deviation of +1,685 (CR = 5.92).

Delay and lack of enthusiasm resulting from the withholding of results for days may have caused the psi-missing reported in my article "Terminal Salience in ESP Performance" (**36**). There were 1,114 runs by children and adults combined, yielding a deviation of −169, which gives a significant CR = 2.53. Also, in an experiment by McMahan and Turner (**27**) in which two subjects attempted to identify the order of decks of cards at different distances and at different times in the future, results were obtained that strongly suggest psi-missing (360 runs with a deviation of −72; CR = 1.89).

In all these cases of delay in informing the subject of his scores it is difficult to think of simple, unconscious negativism. Rather, it seems most plausible to suspect that the subjects experienced some frustration and were affected adversely by the conditions of the test imposed upon them, but were still, on the whole, positively oriented. All were volunteer subjects and the interest of many was very keen, but there can be subordinate motivation which creates annoyance and affects the response even while positive motivation dominates the total behavior.

VII. Psi-Missing and Personality Measurements

Once the psi-missing effect is taken seriously, one of the first questions to arise is that of whether there is any personality state, trait, or type that is related to its occurrence. For example, are there individuals given to systematic unconscious errors of judgment in general in the kind of situation the psi test represents? Or are there more or less transient personality states such as moods and attitudes, or more enduring traits and complexes that might generally favor negative deviations in psi tests in spite of conscious efforts to score positively? The studies of personality correlates of psi processes have made a beginning toward answering these questions. As will be seen, these studies all relate closely to the problem of the occurrence of psi-missing effects.

Most of the work done on the personality correlates of psi capacity is associated with the names of Schmeidler, Stuart, and Humphrey. In their researches estimates of personality traits or states were used to classify ESP subjects and separate them into groups having differentiated scoring rates that could be compared. Generally speaking, the low-scoring groups thus independently separated have gone so low as to give negative deviations. For the most part the negative deviations have been almost as large for the low-scoring group as the positive deviations have been for the other. It would definitely appear that almost all these personality separations have been dealing with the psi-missing factor or factors rather than with the amount of psi capacity represented by the groups separated.

Psi-Missing and a Skeptical Attitude; Factor of Adjustment

Schmeidler's first separation of ESP subjects on the basis of psychological factors, as already mentioned, had to do with attitude toward ESP. The subject (asked in advance whether he accepted the possibility that he could get ESP results in the test) was classified as a "sheep" or a "goat." In Schmeidler's published work (**42**) there are reports on 466 sheep and 317 goats. The 4,248 runs of the former yielded a deviation of +565 and the 3,348 runs of the goats, one of −306. The difference gives a CR of 4.85, which is quite significant. There is also a marked consistency throughout the number of repetitions of the experiment which this long series represents.

Schmeidler found further that by using the Rorschach method to separate the well adjusted from the poorly adjusted subjects, the sheep and goats for the well adjusted group could be still further separated; that is, the well adjusted sheep scored at a higher rate than the poorly adjusted, and the goats who were well adjusted scored still further below the mean (gave a larger negative deviation) than did the poorly adjusted. This separation was extended further yet in its efficiency by Schmeidler's use of seven discriminative Rorschach signs which she worked out for this purpose. These extensions of the difference between sheep and goats at first suggested that while the sheep-goat classification separates the subjects doing psi-missing from those scoring positively, the Rorschach criteria separate out those who are exercising more psi capacity proper. Further analysis of the results of the poorly adjusted subjects, however (**19**), has only recently brought to light new subtleties of the psi-missing effect. It turns out that the poorly adjusted also show ESP ability but require a further separation to distinguish those producing psi-missing from those doing normal positive hitting. As it is, they cancel each other's deviations. Rorschach methods, then, have helped to refine the sheep-goat separation with regard to psi-missing rather than to differentiate the *amounts* of ESP capacity registered.

It is well to keep in mind that any pooled total of psi test data may conceal a great deal of cancellation, variation, and complexity. Many conflicting trends can become lost in the totaling operation. Schmeidler's paper "Position Effects as Psychological Phenomena" (**43**), offers an interesting suggestion of what is possible in obscuring important data. In this report Schmeidler shows that the goats, whose total deviation was *negative* (although this section is not significant by itself), actually scored *above* the chance average in the first and last segments in the run (+10 and +30), thus giving a U-shaped curve of hits. The three middle segments, however, were so far over the negative side (−26, −24, −24) that the total deviation was below the mean chance line (−34 in 574 runs). The analysis of the rest of the goat records for position effects would be of interest; but this sampling can serve at least as a warning against too ready acceptance of a simple hypothesis of negativism to explain the psi-missing results of the goats. They would hardly be expected to give U-curves with the ends above the mean if the performance

was the result of a persistent negative attitude. However, since the sheep-goat attitude cannot be ignored as a present factor, a combination hypothesis is needed that includes both motivation and cognitive distortion, along with other factors such as those that produce position effects.

Negative Deviation and Frustration Tests

Working with the Rosenzweig Picture-Frustration Study, Eilbert and Schmeidler (**8**) and Schmeidler herself in another research (**47**) found a relationship indicated between the level of scoring in ESP tests and the subject's reaction to frustration as measured by the Study. Those who tended to turn their aggression outward made lower total ESP scores and deviated negatively from the mean, while subjects whose aggression was expressed less overtly scored on the positive side as a group. Eilbert and Schmeidler found, too, that on the basis of their work habits their subjects could with some success be separated into groups scoring above and those scoring below mean chance expectation. Judgments were based on such categories as "egocentric, goal-driven, task-oriented."

Expansion-Compression in Relation to Psi-Missing

The work of Humphrey (**16, 17**) on personality measures in relation to ESP shows a similar picture of success in differentiation between positive and negative trends of deviation. Humphrey used drawings made by subjects in ESP tests as a basis for separating high and low scorers. She judged the drawings for the amount of "expansion" and "compression" shown by their form quality. It was found that in the clairvoyance test scores on the drawings themselves the more compressive subjects produced a significant negative deviation. The expansive subjects yielded a positive deviation that is significantly different from the compressive group deviation. There is, however, the still unexplained, though interesting, finding that when the drawings tests were made under the GESP condition, these signs of the deviation given by the expansive and compressive groups were reversed. To the subject this method meant telepathy, even though clairvoyance was still possible. In this reverse case it was the expansives that gave the negative deviation (and it too was independently significant). In both these instances of drawings tests. that is, clairvoyance and GESP, psi-missing was a prominent factor.

Humphrey (**15**) had observed that the use of drawings to estimate the degree of the subjects' expansion-compression had some value in separating the high from the low scorers in card tests of clairvoyance, though much less efficiently than when, as in the work just mentioned, the drawings themselves were also the ESP responses. As it was, in the card tests on 29 series of clairvoyance in which this separation has been attempted the 955 runs by expansives gave a deviation of +62 while 1,949 runs by compressives gave a deviation of −51. But again, as with Schmeidler's poorly adjusted subjects, there is need for a further breakdown to get at the extent of the psi-missing effect. Here, analysis of the position effects on the page, combined with the measure of the compressive quality of the drawings, brings out significant psi-missing. The compressive subjects show a quarter-page distribution of hits in which the upper left quarter has a very significant negative deviation (with a CR of 3.60) while the upper right has a significant positive deviation (CR = 2.6). Consequently the measure of compressiveness taken alone can be said only to have separated out those subjects who *began* their record sheet (upper left quarter) with a psi-missing performance. Other factors took over in determining the record for the rest of the page.

In view of the fact that Humphrey found evidence of change from expansion to compression (and changes in the reverse direction too) from one test to another, the expansion-compression (E-C) measure is evidently not dealing with an enduring personality factor; likewise, the reversal of direction of score deviation in the GESP drawings test from that of the clairvoyance test, both for the expansive and the compressive subjects, makes it appear that the transient test situation determines the degree of expansion and compression.[7] The same may, of course, be said of the sheep-goat differentiation in the Schmeidler work. Accordingly, it is only in the broad use of the word that these may be called personality measures; but in any case they appear to be psychological states important in psi performance. Their importance evidently has to do, not with the operation of ESP, but with the question as to the side of the mean on which the deviation will be produced in the score totals; in other words, whether or not psi-*missing* will operate instead of normal psi-*hitting*.

[7] This reversal was confirmed by McMahan (**23**) in Series V of her pure telepathy experiments. Negative scoring compressives on clairvoyance became positive in the telepathy tests.

Perhaps the best clue given by the expansion-compression work to the problem of psi-missing is the fact that in the clairvoyance drawings tests Humphrey found that the compressives who produced the significant negative deviation also showed a significant backward displacement, and in this displacement the deviation was *positive*. In other words, the subject in avoiding the target was concentrating on the hitting of the just-preceding stimulus drawing. This helps to make more plausible the suggestion of a persistent cognitive error or "bad aim" as an explanation of psi-missing. In this case the subject was blindly misdirecting his psi efforts toward getting the drawing preceding the nominal target.[8]

Interest Pattern as Related to Psi-Missing Effects

Stuart's (**55**) introduction of interest tests provided another basis for the separation of high and low scorers, and again this means subjects who scored on opposite sides of the mean. Stuart developed an interest test on the basis of criterion series of ESP tests. When Humphrey later applied this to a total of 32 new ESP card test series (**15**) it resulted in a significant separation of high and low scorers. According to this interest-scale separation, subjects fall into two groups known as "midrange" and "extremes"; the subjects of the midrange are the more temperate in likes and dislikes and those of the other group are more extreme. A total of 1,687 runs produced by midrange subjects scored a deviation of +188; 1,716 runs by the extremes gave a deviation of −202; the CR of the difference is 3.34.

A somewhat less effective but different type of separation was based upon the Stuart Interest Inventory using a selection of 14 points from the interest test which seemed to offer the best basis for separation on the criterion series. On the 29 ESP card series to which it has been applied by Humphrey (**14**) this test has given a separation that is suggestive but not significant (CR of difference = 1.90). However, a combination of the two methods of separation based on the Stuart inventory offers a much better rate of separation than either one alone. For the 694 runs of subjects who are midrange and also in the "high" group on the 14-point scale, the deviation is +153; while for the 858 extreme-"low" combination runs

[8] The scoring method in the drawings tests makes a negative deviation a somewhat different case from that of the card tests, but does not alter the case entirely. The general significance of the point will, I think, still hold. The full discussion of the technicalities would require too much space.

the deviation is −98. The separation here gives a higher rate of deviation on either side of the mean, and although the number of runs is greatly reduced, the difference still gives a significant CR of 3.27.

In a recent publication (**12**) Humphrey has combined all three of these measures for the total range of series to which they are applicable and finds that the 38 expansive-midrange-"high" subjects did 77 runs with a deviation of +40, while the 90 compressive-extreme-"low" subjects produced 184 runs with a significantly large negative deviation (−72). This further separation resulted in a larger deviation per run than any combination of two of the measures or any single measure alone would yield in selecting out subjects from the group. Humphrey concludes from this and other supporting data that the three measures deal with different functions or qualities of the subjects' personalities. It may be added here that they are all three quite evidently not separating subjects on the basis of the *amount* of ESP shown but rather on whether they tend to ***hit or to miss*** the target better than chance. Yet while they are dealing only with the sign of deviation and not with the differences in amount of ESP as such, it is plain that without these separating criteria the ESP that is evidently operating would have been almost completely obscured and lost.

Security, Adjustment, and Extraversion as Related to Size and Sign of Deviation

Another incidental, though probably important, item belongs here in connection with combining the expansion-compression (E-C) measure with the Maslow security-insecurity questionnaire. Humphrey (**51, 52, 56**) found that it was the secure subjects who were most effectively separated by the E-C rating both in clairvoyance card and drawings tests. Among the insecure subjects, the expansives and compressives were not significantly different, but of the secure subjects the expansives in *both* card and drawings tests gave positive ESP scores, while the compressives in both types of tests scored well below the chance average. The difference in scoring rates for the expansive-secure and compressive-secure subjects was significant for both kinds of ESP test. It should be recalled that it was the adjustment rating as measured by Rorschach that gave Schmeidler her best separation between scoring rates of the sheep and the goats.

Among the sheep the well adjusted gave the larger positive deviation and the well adjusted goats gave the larger negative deviation.

The Bell Adjustment Inventory has only once been applied to ESP test subjects as yet. Lawrence (**22**) found in ESP card tests that 440 runs produced by subjects rated on the inventory as having "excellent" to "average" emotional adjustment yielded a deviation of +79 while the 160 runs of subjects rated as unsatisfactory in their emotional adjustment produced a deviation of −43. This latter figure represents a higher rate of deviation per run (−.27) than the positive one (+.18), and the difference is marginally significant. As far as they go, these results suggest that adjustment is more concerned with sign of deviation than with size, as does also the following paragraph on the Bernreuter.

Humphrey (**13**), on the basis of an earlier experiment in which she found a relation indicated between ESP scoring and introversion-extraversion as measured by the Bernreuter Personality Inventory, applied that inventory to the project of separating high- and low-scoring subjects in the two other experimental series in which it had been used. In these she found that the 96 subjects rated as extraverts gave in their 2,316 runs a deviation of +294, and the 64 introverts gave in their 1,399 runs a deviation of −230. The CR's of each group, as well as that of the difference, are significant. Again, then, the negative deviation contributed by the introverts should be kept in view in considering the relation of adjustment to psi-missing (as compared to the amount of psi capacity).[9]

From the investigations of Humphrey and Stuart, as well as from those of Schmeidler, one gets on the whole the impression that the psi-missing factor is a pretty complex part of the psi test operation. Certain traits and psychological states seem to be associated with it in certain test situations (and not in others), but there are also signs that we do not yet have all the factors that contribute to negative deviations.

Psi-Missing and Personality Types, Traits, and States

Thus far none of the personality studies have turned up a personality type that consistently indulges in psi-missing in the tests. When Shulman (**50**) and Van Wiemokly published their test results

[9] A suggestion of a quantitative relation of ESP to introversion-extraversion rating has recently emerged from Humphrey's analyses, enough to encourage further inquiry.

on involutional melancholia patients, both obtaining a negative deviation with patients of that classification (their results taken together approximating significance), it looked as if there might be associated with that disorder a personality state that generated psi-missing. Price (2), however, found involutional melancholia patients to average above mean chance expectation and the matter therefore stands in doubt. Diagnoses can be questioned, and the influence of the experimenter in handling the subjects can be recognized as a factor of possibly greater importance for ESP orientation than the psychotic personality characteristics of the patient.

What does seem to emerge from the personality studies is the fact that certain more or less temporary states are more likely to be found connected or associated with psi-missing effects. For the most productive "psi-missers" (and there is much to learn from cultivating psi-missing as an objective) one would apparently do well to choose, from among the more normally secure subjects, those who are introverted doubters and who are most given to extreme likes and dislikes, and then thrust them into such a clairvoyance test situation as will make them highly compressive. Delayed checking and long runs should help, a two-task experiment likewise. Probably, however, any subject would exhibit psi-missing if the test situation sufficiently confined, strained, and frustrated him without completely crushing his will to perform successfully.

On the other hand, if a positive effect of psi is desired, it would be best to start with subjects who are eager to see whether they cannot beat "chance" in the tests and, of course, those who recognize this as a reasonable possibility. If they are normally secure, extraverted, well adjusted, temperate, and intelligent persons, the chances are better. Now if the experimenter puts them at ease, keeps them from feeling cramped by the procedure, does not overwork or bore them, and avoids inducing a critical attitude toward the test situation, he is very likely to get a positive deviation. If in addition the experimenter is deeply interested himself and his subjects are inspired to strong enthusiasm for high scores so that they forget everything else for the moment, they probably will score high, for a while at least. The mental state will not last long anyhow.

In a general way, then, it can be said that it is the more transient states of mind that are responsible for psi-missing. Perhaps a few

of these related states are enduring enough to merit the status of traits (introversion, interest pattern tendencies, etc.); so it can be said, at least suggested, that psi-missing owes much to certain *states,* something to certain *traits,* but nothing we know of as yet to *type* characteristics.

VIII. Psi-Missing in General Perspective

The foregoing analyses, then, show that the psi-missing factor has been a highly obstructive one. Had it not been at work, nearly all of these negative deviations would presumably have been added onto the evidence of psi yielded by positive deviations. More important, perhaps, is the inference that there must be a great deal of psi-missing still undetected. Obviously, so long as this disruptive factor is ranging loose and uncontrolled, even unsuspected, in the experimental field, no research is safe from it.

From the point of view of personality, however, and the place which psi must occupy in its total organization, this psi-missing and all the conditions and processes that produce it are of importance. In the larger view, it is possible to see these disturbances as part of the problem of the nature of psi after all. Psi capacity is not a detached function, but is rather the personality operating in a certain way in and out of psi tests; accordingly there could be no proper understanding of psi that ignored this reversing mechanism of psi-missing.

Moreover, through recognizing the psi-missing effect we should be a good step forward toward eventual understanding of the major function itself. And at the very least the mere identification of an element so hazardous to test performance in parapsychology as psi-missing may help to initiate further analytical studies in the search for elements in the total functioning of psi in personal life and action. If, now, the most is made of what is known about psi-missing, the future investigation of psi capacities will never be so hard again as it has been in the past.

Some insight into the problem may well be expected from the study of those spontaneous psi experiences that bear most closely on psi-missing. Cognitive distortions, as well as peculiar motivational twists, need to be examined in large numbers of classified case reports to see whether mechanisms are discoverable that suggest a psi-missing factor. A few reports suggest that in rare individuals there

is a fairly regular tendency to have experiences that have to be interpreted in reverse (for example, a euphoric state of anticipation that must be taken as a portent of bad news, and the old saying that a dream of a wedding means there will be a funeral—situations which may have arisen from some general experience of reversed meaning in psi experiences). Studies are under way that should advance our knowledge in this quarter.

How specific an effect is psi-missing? Is there anything like it in psychology? If there were any equivalent to a psi-missing factor obviously active in non-psi operations of the personality, the fact would presumably have been brought forward long ago. There is of course no need to consider in connection with psi-missing all the many effects of unconscious negative motivation on cognitive processes. The literature of abnormal psychology is rich in such manifestations. However, interest would definitely center around any examples of negative deviations from expectation to be encountered in the normally motivated performances in judgmental tests, memory tests, learning, and the like. Let us make sure at least that motivated missing does not obscure such examples if there are any.

There are, indeed, suggestions from the studies reported above that psi-missing is probably a more general function in the personality than these psi-test effects would represent. Why should it not be? It should be much less surprising to find such an unconscious reversing function as general and normal than not. (The degree or extent of its activity might depend on certain personality trait developments.) If there is, and it can do to the individual in other areas of action what we have seen it do in psi experiments, then general psychology had better inquire further into the matter. But the inquiry needs to be on subtle lines; obviously the psi-missing effect is an obscure one.

Résumé and Discussion of the Way Psi-Missing May Work

It is plain from the review given and the comments made in passing that the operation of psi-missing is not yet understood, even though it is a fairly common occurrence in the research. A simple explanation in terms of unconscious negativism on the part of the subject is not enough to explain very many of the examples reviewed and there is no certainty that it is the explanation of any one of them. Not that there is any difficulty in supposing that if the subject is

unconsciously trying to avoid hitting the symbols, he can produce as large a deviation in the negative direction as he normally produces in the positive direction; that has been shown to be possible with conscious "low-aim" tests. Rather, it is difficult to suppose that in many instances subjects have sufficient negative conation to produce the range of results on record.

Also there are, as the review shows, many examples of change of deviation sign in which it is not reasonable to suppose that the direction of motivation of the subject has changed at all. When in the course of a long run he recurrently begins above the mean chance expectation line and then in run after run falls below toward the end, it hardly looks like a shift of attitude. The finding that caffeine (or something associated with it) kept the run from diving below the "chance" line in the PK series mentioned above argues further against giving any primary role to conation. It would be definitely unsound to infer that the subjects, even unconsciously, do not *want* to score positively, for example, on the less favored of two conditions being compared in a psi experiment, or that the groups of psi-missing subjects who are separated out by the personality measures score consistently below expectation because they have an unconscious negative attitude. It should be remembered that Schmeidler found a U-curve within the run in the hit distribution of her goats, and that the beginning and end of the run made by her "goats" were above mean chance expectation. A simple theory of unconscious negativism, then, will not explain much of the psi-missing evidence.

The following view, however, is suggested by the facts surveyed. All that is necessary for psi-missing is that under strain or other disturbance the subject modify the particular function that normally gives him his psi results so that the effect is systematically to pass over the right response and accept another. This is the hypothesis of systematic cognitive error, or simply psi error.

There are, as suggested at the outset, many conceivable causes that could produce the psi-missing effect; and different ones may influence different subjects. In the case of a subject who, for example, becomes inclined to displacement (ESP response to a card adjacent to the target, or to any other card in the run) the very tendency to direct psi toward a certain position (say the next card ahead) can automatically involve avoidance of the regular target. (This need

not be due to rejection of the target symbol as such; habits such as avoidance of doubling would be sufficient to do it along with the forward displacement.) Or again, a subject may be forced by some tension the test produces to distrust the semi-spontaneous way of choosing his response symbol; when he changes to a method which may seem safer to him (perhaps, for example, waiting until one image appears brighter than the others) he is likely to keep as far away from the *rejected* method as possible, hence to avoid the symbol it would furnish. All this is of course unconscious and not at all logical. Operating blindly, yet systematically, the very purpose of the subject to grope his way to the right choice of symbols leads him to overreach and unwittingly to ignore the correct response in abandoning the device that gives it. Any small shift of motivation might suffice to do this by introducing a misguided change of practice in choosing symbol sequence.

Thus far the discussion of the way psi-missing may occur has been confined to ESP; but a word is in order on psi-missing in PK tests as well, for it does occur. However, the hypothesis suggested for PK is not very different from that offered for psi-missing in ESP. Some cognitive orientation is needed in PK; somewhere, sometime, and somehow a die (or many dice) is to be influenced the right amount and in the right direction. All cognition operates unconsciously too, and any slight tension or influence exerted on it should be expected to affect it much as it affects ESP itself. Change to another mental device or practice here too induces a systematic by-passing of the one abandoned and the effects it produces, probably the same as it does with ESP.

Psi-Missing and the Operation of Normal Psi

What, then, is the difference between psi-missing and the general missing in psi tests that even in a positively scoring subject prevents him from getting perfect scores? To answer this adequately would require knowledge of all the limiting factors in the functioning of psi. But of these little is known. We do know that basically there has to be an active process that is both cognitive and conative in character. The subject is striving to do something involving "knowledge" and this striving varies from time to time and from individual to individual. This variation in effort alone would affect success. There are too, presumably, individual differences in psi capacity it-

self, regardless of the subject's conation. Thus, whether or not a subject will succeed may depend to start with either upon native psi capacity or upon the amount of striving aroused in him, or upon both.

Next there is the difficulty of getting psi to thrust through the competing processes, the inhibiting habits, and whatever other unknown barriers it has to cross in order to come into consciousness, to some form of expression or performance, whatever the end result is to be. Much depends on the timing. A subject may in the midst of a difficult mathematical computation be relatively impervious to the operation of his psi function. Something, too, is determined by training. The subject, as a result of intellectual training that led him to reject the possibility of such a capacity, may have established an effective censorship against psi experience. Or he may be a highly integrated creature of habit and reason who has cultivated no gaps in the trained procedures of his mental life. At any rate, *psi receptivity* will be found to be another variable of large range that could presumably determine whether or not a psi test would be successful or a psi experience possible.

There are, then, at least three distinguishable parts to the whole psi operation: The first has to do with the requisite amount of psi, the second with the strength of the purpose to exercise it, and the third with the penetration of the mental barriers it encounters. So far as these three elements go, any psi response that is registered should be a positive one. These factors do not produce psi-missing. But whether or not there will be only a few responses above the chance mean in the run of trials, or whether there will be a perfect score, will depend upon at least these three areas.

The principal reason why subjects do not all score 25 on every run of ESP is that psi is unconscious—hence the over-all inability of the subject to know whether what comes into consciousness is a true product of psi ability or merely the working of the chain of associative symbolism into which the psi product has to project itself to achieve expression.

Where the psi-missing factor comes in is another part of the organization. It has nothing to do with the force of the motivation. It is not a matter of how much psi capacity the individual has, and not at all a question of how well psi gets through the barrier to consciousness. It is a fourth consideration. Once a psi operation has

been achieved, and before it reaches conscious expression or overt response, it has been converted either into a hit or into a miss. It has come to conscious expression in the way intended (a hit), or it has been distorted, systematically or not, in the course of its transition across the barrier (in any case a miss). If the distortion has been systematic, if the subject has unconsciously used his psi function to avoid the target time after time, it is psi-missing, and a negative deviation accumulates that can become reliably significant.

For this psi-missing effect we need suppose no purposively functioning censor or interpreter, but we can all recognize that in the operation many different conative tensions may affect the course of the translation of a psi effect into oblique channels of response that would result in target avoidance. Obviously, if this picture is correct, then the discovery of these distorting tensions and secondary motivating factors should help us to understand psi-missing and probably to bring it under control.

Some Practical Suggestions for Dealing with Psi-Missing

If it is going to be feasible to get psi-missing under control by the research worker in the future, there are some practical needs that should be faced. Some further attention may profitably be given to extending the use of special statistical techniques and analytic procedures to deal with psi test results independently of the sign of deviation, especially in the evaluation of results of comparative studies. For example, in personality researches correlation with and without deviation sign is needed in more instances to see whether it is the sign itself that is important (and psi-missing the factor involved), or whether it is the amount of psi, or still a third factor, that is related to the variable being studied.

There is a need also for further analyses of more refined character to determine the extent to which this cancelling effect of psi-missing is diverting attention from the full range of psi activity. The investigator who has had long experience in parapsychology has in many cases come to recognize that a total deviation that is insignificant does not by any means justify the conclusion that no psi activity has taken place. In many series closer examination has shown that evidence of psi has been obscured because psi-missing in one section of the experiment has cancelled the positive deviation

produced by the rest. The alert experimenter today will keep in his perspective, not only the various ways in which psi-missing has left its mark on the records, but also the conditions that have produced it. And he will of course not assume that these are all known as yet.

Perhaps the most practical need on the psi-missing problem is for a device to control it—at least unless and until it can be eliminated. In a number of cases showing psi-missing effects the subject seemed to orient himself all the more positively toward one part of an experiment if there were another which induced a negative deviation; in fact, both deviations appeared to have been increased by the contrast. The idea (still in need of further exploration) is that it may be well to make the psi test a double one, with a secondary aspect added merely to drain off by contrast all the psi-missing effect that can thus be attracted, throwing the subject by reaction toward the favored and primary aspect of the test. This hypothetical plan can be developed in different ways and can be applied to psychological as distinguished from experimental aspects of the test. Two experimenters or two features contrasting on different lines can be introduced to separate the positive and negative scoring tendencies of the subject as widely as possible.

It will naturally be most profitable to try to grasp the nature and control of the psi-missing factor by more direct research and a closer study of the general place of psi function in the total personality system. In the meantime, however, there is no need to go on blindly allowing this unruly element to play havoc with the subject's performance and with the interpretation of results. If practical procedures *can* be set up in the experiment to keep psi-missing to some degree in its place, if analytic and statistical safeguards *can* be applied to minimize the danger that it will mislead evaluation and interpretation, and if all the while efforts *can* be directed toward discovery of ways of understanding it and either eliminating or controlling it at the source, the course of psi research may be expected to run not a little smoother and faster.

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BOOK REVIEWS

NATURE, MIND, AND DEATH. By C. J. Ducasse. LaSalle, Illinois: Open Court, 1951. Pp. 502 + xix.

This is an important and very interesting book. It is nothing less than a comprehensive discussion of the relation between mind and matter, and more particularly the relation between mind and body. This vast subject is treated in all its aspects, including the paranormal ones. Prof. Ducasse is not only a distinguished philosopher; he is also one of the few philosophers who are interested in parapsychology and familiar with the literature of the subject. I think that the whole of Part IV of this book ("The Mind-Body Relation and the Possibility of a Life after Death") ought to be compulsory reading for all students of parapsychology; and the same applies to the discussion of precognition and freedom in Chapter 11. But to understand what Prof. Ducasse has to say on those two subjects, one must have some idea of the philosophical context in which he says it and of the philosophical terminology he is using. His views about the philosophy of parapsychology are integral parts of his philosophy as a whole. To do justice to them, something must be said about his treatment of the traditional philosophical problems concerning the relation of mind and matter, problems which arise from reflection upon "normal" experience and upon the concepts of common sense and of the orthodox sciences. I shall try to make that part of my discussion as untechnical as possible, but I am afraid that a certain amount of technicality is unavoidable.

I shall only mention two points about Part I ("Philosophy: Its Scope, Subject Matter and Utility"). First, Prof. Ducasse maintains that the problems of philosophy are essentially semantical, concerned, that is, with questions of meaning rather than with problems of fact. But that conception of philosophy does not have the inhibitory effect upon him which it has had upon some other thinkers. It does not at all prevent him from discussing the traditional questions of metaphysics and epistemology and offering answers to them. It only has the salutary effect of making him careful to disentangle the ambiguities and verbal confusions which have caused those ques-

tions to appear even more perplexing than they are. Secondly, in connection with the words "real" and "really" he draws an interesting distinction between ontological *hypotheses* and ontological *positions*. An ontological "position" (he also calls it a "commitment") is not something true or false. It is simply a rule which one adopts, tacitly or not, for deciding what things are to count as basic or primary: for example, the rule that to be real is to be perceptually public or implicit in what is so. Looked at in another way, an ontological position is a limitation of one's field of interest or inquiry. But what one excludes from one's field of inquiry may nevertheless happen to exist.

Part II ("Fundamental Categories") is mainly concerned with the concepts of causation and substance. Prof. Ducasse rejects the Regularity Analysis of causation which most empirically minded philosophers since Hume's time have accepted. Instead, he suggests that by "the cause of an event E" we mean "the only change introduced into the situation immediately before E occurred." In the same chapter there are some interesting remarks on action at a distance. In effect Prof. Ducasse accepts Thomas Carlyle's dictum: "Nothing can act but where it is. With all my heart! But where is it?" In short, he suggests that such phrases as "where a thing X is," or "the boundaries of a thing X" have themselves to be defined in causal terms. "A thing *extends* to wherever it can act directly, i.e., act without needing therefore to act on some intermediary other thing" (p. 136, my italics). Unfortunately Prof. Ducasse does not mention PK at this point, though he does elsewhere. It would seem to follow from his argument that in PK the mind of the agent is extended, and even perhaps has a shape (e.g., if it produces simultaneous PK effects upon a number of objects in different places).

In the last chapter of Part II (Chapter 11) there is an elaborate discussion of freedom and determinism. Of course both "free" and "determined" are highly ambiguous words, as Prof. Ducasse points out. And he argues that there is *a* sense of "free" and *a* sense of "determined" in which freedom and determinism are perfectly compatible. He accepts determinism in the sense that every event has a cause; indeed, he claims that this proposition can be proved and is not just a postulate. And he accepts freedom in the plain man's sense (which is also Locke's): that "I am free to do X" means "I

can do X *if* I will to do it"; which is in turn equivalent to "a volition by me to do X would now suffice to cause the occurrence of X."

Will this conclusion stand if precognition is taken into account? Prof. Ducasse claims that it will. He points out that even when precognition is veridical, it still does not amount to *knowledge* of a future event. To have knowledge, it is not enough that one should believe something which is in fact true; one must also believe it on grounds which are sufficient to establish that it is true. "Precognition," then, is a misleading term to use, because it postulates "not only that the experience is veridical, but also that it includes evidence of its own veridicality" (p. 205). This criticism seems to me rather harsh. It would be a fair objection to the use of the term "foreknowledge," but the word "cognition" is customarily used by philosophers and psychologists in a much wider sense than "knowledge." Erroneous beliefs, for instance, are usually regarded as cognitions or cognitive states. However, Prof. Ducasse thinks that the term "preperception" would be better, since everyone agrees that perception is fallible. He then considers two well-known examples, the case of Mrs. Atlay and the pig, and of Lady Z. and the coachman. His argument is roughly this: If Mrs. Atlay's dream had caused her to take effective precautions for keeping the pig in its sty, then of course her dream would not have been a veridical preperception. And she was free to take those precautions, in the sense that she could have taken them *if* she had willed to do so. She did not will to take them because she did not believe that the event preperceived was "a risk," (i.e., was genuinely likely to happen unless something was done to prevent it). It is also conceivable, no doubt, that even though she *had* taken precautions (by locking the pigsty door) the precautions might have turned out ineffective and that the dream would still have been verified. But this is not incompatible with her freedom either. She did have freedom to take precautions against the pig's escape. But she did not have freedom to make those precautions one hundred percent effective, because no one in any case has as much freedom as this, and no sensible person would claim that he had. Even though the door was locked, the walls of the pigsty might have been overthrown by a hurricane or an earthquake, and then the pig would still have got out. " 'Tis not in mortals to *command* success"—at any rate when the events they wish to bring about, or prevent, are events in

objects outside their own bodies. Whether we are satisfied with this solution or not (and of course the extreme Libertarian will not be) I think we must all agree that the puzzle about freedom and precognition does arise at least partly from two confusions: first from identifying precognition (preperception) with *foreknowledge,* and secondly from failing to recognize that the word ***freedom*** has many different senses. Prof. Ducasse has at any rate cleared up these two confusions.

From causation, Prof. Ducasse passes to substance. The nature of a substance, he says, analyzes into properties and relations of properties. It is important to understand what he means by the word "property," because this concept plays a fundamental part in his theory of mind, matter, and the relations between them. By "properties" he means *causal* properties or capacities, what Locke called "powers" and some modern philosophers call "dispositions." Properties, in this event, must be carefully distinguished from qualities. It follows that words like "bitter" "hot," "red," etc. are systematically ambiguous. As applied to a taste-sensation, the word "bitter" stands for a quality of the sensation itself. As applied to a substance, quinine for example, it has an entirely different meaning. Here it stands for a property—a causal capacity or power which quinine has of producing such sensations in normal human percipients. Such properties are always analyzable into *laws,* or general hypothetical propositions; e.g., if quinine is applied to the tastebuds of a normal human being, he experiences sensations with a bitter quality. And the words we commonly use for describing different sorts of substances (e.g., bread, water) refer in the end to the different sorts of properties (powers, causal capacities) which are characteristic of these substances. Moreover, according to Prof. Ducasse, we should know nothing of material substances at all, if they did not possess properties of the special sort which he calls physico-psychical—capacities to produce sensations in us. But there are also physico-physical capacities, psycho-psychical ones, and psycho-physical ones; the last two are characteristic of mental substances.

What else does a substance consist of besides properties in this sense? Locke, who said that powers (Prof. Ducasse's properties) "make up a great part of our ideas of substances," maintained also

that there is always a substratum, "a something we know not what" which *has* those powers. Prof. Ducasse will have nothing to do with unknowable substrata. And just for that reason, I am not sure whether he was wise to use the word "substance" at all. It seems to me that W. F. Johnson's term "continuant" (which does not have those implications) would have been a better one for his purpose. However this may be, when we ask Prof. Ducasse what else a substance consists of, over and above a set of properties or causal capacities, his answer is, I think, just "events." Here an important distinction emerges between physical substances and mental ones. In physical substances, the only events we know of are what might be called "second order events"—events which are themselves *changes* of capacities, for example, melting, bending, loss or gain of electric charge. Even motion itself is nothing but a name for the fact that a set of properties which was formerly at place P_1 is now at place P_2. But with minds it is different. The capacities of minds do of course change. At 2:00 p.m. I have the capacity of recalling what you have just said, and at 2:05 p.m. I have lost it. But in minds there are in addition first order events, which do not just consist in changes of capacities. Those are the events which Prof. Ducasse calls "intuitions"—sensations and other introspectable experiences. And those, he says, are known directly, in the very act of experiencing them. Various changes of capacities (whether mental or physical) are known only indirectly, by means of an inductive inference; indeed, strictly speaking, they are believed in or postulated, rather than known.

A word must now be said on Prof. Ducasse's theory of sensation and perception. As to sensation, he defends a "connate accusative" theory ("internal accusative" is the more usual phrase, at least among British philosophers). According to this, a sensation of blue is not the awareness of a blue particular, a sense-datum or sensum characterized by blueness. That would be an "external accusative" theory. On the contrary, having a sensation of blue just consists of sensing *bluely*—the word "blue" is just a way of describing *how* we are sensing.

Perception, according to Prof. Ducasse, is something quite different. It is an activity of interpretation, normally automatic and unreflective, by which sensations are interpreted as effects of extra-

mental causes. Some of our "intuitions," we find, are caused by other "intuitions" of ours (e.g., volitions by emotions). But intuitions of the special sort called sensations have no intra-mental causes. We know, however, that they must have causes of some kind or other. So we take them to be effects of extra-mental causes. And this is both the origin and the justification of our belief in the existence of matter. It follows (as has been remarked already) that we could not know anything about material substances unless they had "physico-psychical" causal properties—powers, as Locke put it, to cause sensations in us. The physico-*physical* properties of material substances—the only properties of them in which the natural sciences are interested and the only ones which materialistic philosophers regard as real—are only known through the physico-*psychical* ones, and indeed must in the end be defined in terms of these. Thus if a piece of metal has the physico-physical property of melting when heated, this amounts to saying that in certain circumstances one set of physico-*psychical* properties (sensation-producing powers) is regularly replaced by another.

There are some difficulties in this theory of perception. First, we notice that Prof. Ducasse's reason for believing in the existence of matter is extremely like Berkeley's reason for *dis*believing in it. Berkeley concluded, from much the same premises, that sensations are directly caused by the wish of God. Secondly, it may be objected to both of them alike that sensation might for all we know have *intra*-mental causes. They might be caused by subconscious or unconscious mental processes in ourselves, as dreams and presumably some hallucinations are. Or again, for all we know, they might be telepathically caused by mental events in other finite minds. It would at least have to be shown, I think, that those alternative explanations do not enable us to predict and control the course of our sensations, whereas the explanation in terms of material causes does. Prof. Ducasse hints at such an argument but does not develop it fully. Finally, we generally suppose that the *immediate* causes of our sensations are events in our own brains. But Prof. Ducasse's theory, as he states it, appears to argue directly from sensations to causes which are not only extra-mental but also outside our own bodies. At any rate this is the natural interpretation of what he says in Chapter 15 (Section 10 and following sections).

Nevertheless, it is of course true that we should know nothing whatever about the physical world unless some objects in it had physico-psychical properties (sensation-producing powers). On reflection, this appears perfectly obvious. But it is one of those obvious truths which we are exceedingly liable to forget, especially in an age when our whole way of thinking is dominated by the prestige and success of the physical sciences. The physical sciences, as also the technology derived from them, do systematically ignore the physico-psychical properties of matter and concentrate our attention on the physico-physical ones. This easily leads us to accept behavioristic theories of human nature without seeing how paradoxical they are. But they *are* exceedingly paradoxical, as Prof. Ducasse points out. Indeed, he goes farther. Behaviorism, he says, is just a proposal to change the meanings of certain familiar words such as "think," "consciousness," "mind," "experience." Thus "what radical behaviorism chooses to call 'mind' is something which in ordinary English would be called *'the behavior peculiar to bodies that have minds'* " (p. 230). Such a proposal cannot of course be called false, any more than true. The antithesis of true and false does not apply to proposals at all. But what *is* false is the allegation that such a procedure is not merely a proposal, but an important scientific discovery. The motive behind the behaviorist's proposal is just a certain ontological "position" or "commitment" (see paragraph 2, above), an optional and arbitrary way of defining the word *real,* whereby nothing is to count as real unless it is "either something susceptible of being perceptually public, or something existentially implicit therein" (p. 235).

Similarly, the epiphenomenalists, though they do not try to change the meaning of the word "consciousness" and other psychological terms, are unconsciously dominated by the ontological position they have taken up. They have resolved that no causal process shall be counted as "real" unless it is either physico-physical or physico-psychical. By this resolution, psycho-psychical causation is automatically excluded from being "real." An emotion, for example, cannot "really," but only apparently, be the cause of a wish. And psycho-physical causation is similarly excluded; a volition cannot "really" cause a bodily movement. But in actual fact our empirical evidence for the two forbidden kinds of causation is just as good as for the two permitted ones.

It will be objected, perhaps, that causal relations between entities so unlike each other as minds and bodies is unintelligible or inexplicable. Prof. Ducasse, much as he differs from Hume on other matters, takes a Humean line about this: quite rightly in my opinion. As Hume remarked, for all we can tell *a priori* "anything can cause anything." Only experience can tell us what causes what. It does tell us quite unambiguously that some mental events cause physical ones, and some physical events cause mental ones. And as to the allegedly "mysterious" character of mind-body interaction, it is no more and no less mysterious than any other sort of causal relation. The question, "*How* does A cause B?" is only meaningful when the causation is remote, not when it is direct. The question does make sense when A and B are united by a causal chain of intermediate events: e.g., "How does pressing the trigger cause the bullet to move?" This is a request to have the intermediate links exhibited to us. But when there are none, it becomes a nonsensical question—one which no longer arises (p. 430).

Indeed, if we ask why one describes a certain body as "one's own" the answer, according to Prof. Ducasse, is just that it is the body with which one's mind directly interacts. Mind-body interaction is thus "analytically true" (p. 429). "My body" just *means* "that body with which my mind interacts." This doctrine has a corollary which Prof. Ducasse does not point out. It is logically possible that I should have several bodies at once; or, if you like, that "my body" should consist of two or more bodies separated by a spatial interval. Again, it is logically possible that sometimes I have several bodies at once, though normally I have only one. This would happen in PK; and if PK always involves clairvoyance, as Prof. Rhine has suggested, the interaction would be *two-sided.* If PK were much more extensive and controllable than it is at present, one should be hard put to it to decide which piece of matter out of several was to be called "Smith's body" or where exactly his body was situated.

We must now consider Prof. Ducasse's doctrine that minds are substances. Just what does this contention amount to? We may notice that he several times refers to the Buddhist doctrine of mind with approval. But the Buddhist *Anatta* doctrine is usually taken

to be the denial, not the assertion, of the substantiality of the ego. Nagasena, in the *Dialogue of King Milindha* from which Prof. Ducasse quotes, would have been very much surprised to hear that he was propounding a substitute theory of mind. It would usually be said that the Buddhists hold a serial theory of the self, as opposed to a substance theory, and indeed that the Buddha was a very great philosopher precisely because he invented the serial theory, many centuries before it was heard of in the West. We notice also that Prof. Ducasse is certainly not maintaining that a mind is an *indivisible* entity. On the contrary, if we consider the total set of capacities which makes up "the nature" of a certain mind, he insists that it is rather a loose-knit system in which there are many parts or sub-systems ("role solvers" as he sometimes calls them). A Freudian complex would be an example, and another would be (perhaps) a mediumistic "control," for instance Mrs. Leonard's control Feda, or the subpersonality Sally in the Sally Beauchamp case. Prof. Ducasse points out that even in the normal mind those mental sub-systems have a good deal of autonomy, and each of them has its own "will-to-experience-and-expression." Consequently, when we come to consider the problem of survival, we shall have to consider the possibility that only a *part* of a given mind survives death and not the whole. All this, I believe, is very good sense. But it is just the kind of theory which substantialist theories of the self usually deny. Again, when Prof. Ducasse says that minds are substances, he is certainly not asserting that in any mind there is an *unchanging* core or nucleus, nor yet that a mind is an extra something over and above the totality of mental sub-systems which are its parts. He sides with the Buddhists on both those points. He will have nothing to do with the "pure ego" theory; and yet this is just what many philosophers who have believed in mental substances have wanted to maintain. I suggest then that what Prof. Ducasse is saying when he propounds his substance theory of minds amounts only to this: First, he is saying that minds have *as* good a claim to the title of "substances" as material objects have; and this seems to be true, but I doubt whether very much is gained by applying the title to either. Secondly he is saying that mental events, or rather those of them which are experiences ("intuitions" in his language) are irreducibly different from any events which happen in the physical world. This

is true too, and of course it is fundamentally important. But the serial theory of mind, the theory of the Buddhists and of Hume and William James, safeguards this essential point just as effectively as the substance theory does, or even better. In making these remarks I am not criticizing Prof. Ducasse's theory of minds, but only the terminological tactics, so to speak, which he has adopted in formulating it. By calling it a "substance" theory, I think he may mislead some of his readers.

In the theory itself there is only one point about which I feel uneasy, and that is his treatment of the very difficult problem of subconscious mental events. He prefers to say that they are *conscious* mental events, but subliminal ones; and what makes them subliminal, if I follow him rightly, is that they are just bare experiences in which there is no element of "interpretation." Supraliminal sensations, for instance, are always interpreted as appearances of some physical object or other. We use them for perceiving the physical world. Subliminal sensations on the other hand are not so used; we just experience them and that is all. But will this account for the elaborate processes of planning and problem-solving which appear to go on beneath the threshold of consciousness? It certainly looks as if intelligence and purpose enter into them. Sometimes the intelligence and purpose are morbid or perverse, as Freud and others have shown. But not always; what about the "inspirations" of the artist or the mathematician when the solution of his problem just comes to him ready-made?

We are now in a position to consider the most interesting and original part of Prof. Ducasse's book, his treatment of the problem of survival in the last three chapters. In Chapter 19 he gives an excellent account of the psychological causes which have made so many people in all ages believe in survival. He points out that when people profess to believe in immortality or to wish for it, they give little thought to the "forever" part of the hypothesis. What really interests them is just survival for some substantial period, perhaps a thousand years or even a hundred. He also points out—what needs pointing out, at least for Western readers—that belief in survival is logically independent of belief in God. The question whether we survive death, in short, is not a *theological* question at all but a ques-

tion of what the universe happens to be like, whether or not there is an omnipotent, omniscient, and perfectly benevolent Supreme Being upon whom the universe depends. In other words, it is a question of empirical fact, though we may not at present have the data which would enable us to answer it.

Prof. Ducasse then discusses the case against survival. The strongest arguments against it appear to be two. The first and most familiar is the empirical argument from the biological sciences, not only physiology but also zoology, which appears to show that the degree of consciousness and intelligence displayed by various species of animals is correlated very closely with the complexity of their respective nervous systems. Yet it is obvious on reflection that these facts are perfectly *compatible* with the dualistic conception of human nature, which regards mind and body as two distinct but interacting entities. The interaction theory does not deny, but on the contrary asserts, that mental events in an embodied mind are influenced by bodily ones; and an interactionist can cheerfully admit that the degree and extent of this influence are whatever they are empirically found to be. If it turns out to be much more extensive than naïve common sense would have expected, this is still perfectly consistent with the hypothesis that a mind can exist without a body. The biological facts only show that if any minds do so exist, they are likely to have lost a good many of the capacities which they exercised in their previous embodied state. This is equivalent to saying, in Prof. Ducasse's terminology, that only *part* of a mind is likely to survive death (if anything does) and not the whole of it. As he puts it later: "That what we may call a man's 'noon-day personality,' i.e., the whole set of capacities ready to function in him as occasion calls for when he is at his best, survives intact the death of his body seems very unlikely indeed" (p. 481).

What of the empirical arguments *for* survival? Prof. Ducasse has nothing very new to say about this, and he does not discuss the evidence in any detail. He insists, rightly I think, that there is a good deal of very respectable empirical evidence which does *prima facie* support the survival hypothesis. But as he points out, there are after all alternative explanations for much of it, once we take into account the existence of paranormal cognitive powers—telepathy, clairvoyance, precognition, and retrocognition. In view of this, he

says, not only is the evidence ambiguous, but even the very question "Does the evidence establish survival?" is itself ambiguous. He concludes that though there is evidence that in some instances *something* survives which appears to be some part, or set of capacities, of the mind whose body has died, "nothing both definite and well evidenced can yet be concluded concerning the actual, as distinguished from the theoretical, possibility of partial survival" (p. 483).

Many writers would have stopped at that point. The interesting thing about Prof. Ducasse is that he does not. For there is another argument *against* the possibility of survival, besides the biological one, and he rightly attaches great weight to it. This is simply the difficulty of conceiving what a disembodied mental life could possibly be like. And that in turn suggests that the very conception of survival is an empty one—indeed not a conception at all. In the last and most interesting chapter of his book, Prof. Ducasse sets himself to answer this argument. He answers it by actually producing a number of different conceptions of survival which he claims to be at once theoretically possible and consistent with the known empirical data. It must be carefully noticed that he does not claim more than this. It is possibilities which he is exploring, and not facts or alleged facts. But it seems to me exceedingly important that such possibilities should be explored. I think that this chapter is a most notable contribution to the philosophy of parapsychology.

Prof. Ducasse begins with the first and simplest of the theoretically possible forms of survival. This would consist just in "the continuation of a single state of consciousness after death" (p. 485). If there were no change in this state of consciousness, no passage of time would be experienced, and in this sense it would be "eternal" (non-temporal) whether or not it was also "everlasting" in the time of an external observer. Prof. Ducasse points out that survival in this sense would not be *personal* survival. But it might conceivably be something much better—if the persisting state of consciousness were what the mystics have described under such names as "Nirvana" or "the peace that passeth understanding." On the other hand, it is also conceivable that it might be a state of unbroken anguish, or indeed any other state of consciousness whatsoever.

A second and perhaps more interesting possibility is what Prof. Ducasse calls "survival as dream-consciousness." This would con-

sist in "images of various kinds of objects and situations in which we play some role—the content of the dream being determined at least in part by conscious or subconscious wishes or fears" (p. 486). He points out that such a kind of survival would account for at least some of the descriptions of the next life which are given in mediumistic communications, "reports as to scenery, houses, associates and occupations virtually duplicating . . . those of ordinary life." It would account for Raymond Lodge's celebrated cigars, which have given so much scandal. One might quite well dream of smoking cigars. Religiously minded critics, we may add, sometimes profess to be shocked by the "materialistic" character of the next life as mediumistic communications describe it. But of course a coherent and continuing dream world would be bound to have just this "materialistic" character, just because dream-images so closely resemble percepts of material objects, though the causal laws which prevailed in it might be more like the laws of Freudian or Jungian psychology than the laws of physics. We notice further that in this conception of survival, there would not be *one* "next world" but many. "Where wish is architect and playwright, diversity of scenery and of drama is naturally to be expected from diversity of persons" (p. 487). And that would account for the fact that some mediumistic reports about the next life deny what others affirm. It would also explain why we are told that deceased persons often find it difficult to realize that they are dead, just as dreamers do not usually realize that they are dreaming.

I have only one point to add. It is theoretically possible that there might be a form of *post mortem* experience in which the "threshold" between conscious and unconscious was removed. All kinds of memories, wishes, and fears which were repressed in our earthly life might then come into consciousness. It is plausible to suppose that the "censorship" which would keep them repressed might cease to operate in a disembodied state. The dream life which would result from this might be an exceedingly unpleasant and terrifying nightmare, very much like the traditional Catholic conception of Purgatory. Such a nightmare might go on for a very long time, until the repressed desires, guilt-feelings, etc. had worked themselves out. The point is worth mentioning because so many people suppose that a belief in survival is accepted merely because it is comforting. I am not sure that it is comforting at all.

This has some bearing on the next conception which Prof. Ducasse propounds, though he paints it in less sombre colors. This is what he calls "survival consciousness as reflective reminiscence." It would consist in "a reviewing of the memories of one's embodied life, of reflection upon them, and of distillation from them of such wisdom as reflection can yield" (p. 487). The *contents* of such a consciousness would consist of memory-images. In this way it would resemble the dream-consciousness described before because the materials of our dream-images come ultimately from memory. But our attitude to these images would be different. We should reflect upon them and ruminate over them instead of experiencing them uncritically just as they come. Prof. Ducasse points out that this process could not be expected to last forever, though it might last for a long time. Our memories, even when we include those which are forgotten in this present embodied life, are after all finite in number.

Thirdly, going back to the dream-like "next life" above described, we can conceive two possible variants of it (p. 488). In addition to the dream-consciousness there might be "critically controlled creative imagination"—the purposive activity characteristic of mental creativeness or discovery. There is also another possibility. It might be that not *all* the images one was aware of were dream-images. Some of them might be caused "externally." A certain amount of telepathy or clairvoyance (or, I suppose, precognition) might intrude into our dream, as indeed happens sometimes in some people's dreams in this present life. Such externally caused images would be resistant to our will, as sensations are now. And this provides us with a way of conceiving how a disembodied mind might be aware of a kind of *objective* "next world"; whereas the purely dream-like "next worlds" hitherto considered were subjective. It would also be a *public* next world, whereas those previously described were private. If we suppose that telepathy operated more freely than it does in our present embodied life (because the pressure of biological needs would have been removed), there might be a close and constant correlation between the images experienced by mind A and those experienced by other minds, B, C, and D; just as there is a close and constant correlation between their sensations now when they are all perceiving the same sort of material objects.

In the next stage of his argument (p. 489) Prof. Ducasse takes

a step which I cannot altogether follow. Having shown that there might conceivably be an *objective* after-death world on the lines just described, he asks what spatial and temporal relations such a world might have to the material world which we now know. His reply is that this objective after-death world "might conceivably be some planet in one of the many other solar systems" (or, I suppose, some planet other than the earth in our own solar system). "But," he adds, "it might equally be this earth over again." And this leads him to a discussion of "survival as transmigration" (the reincarnation theory) which occupies the remainder of the chapter.

This transition is difficult to follow. Somehow or other we have slipped from a discussion of *dis*embodied mind into a discussion of *re*-embodied mind. I think that at this point we have gone too far and too fast. We must retrace our steps a little and go back to the "objective" next world discussed in the last paragraph but one. For this, when first introduced, was presented to us as a possible "next" world, that is, a world which *dis*embodied minds might experience. And when Prof. Ducasse asks what relation in space or time such a world might have to the material world which we now know, another and quite different answer is possible: "None at all, or at any rate no spatial relation." There is no *a priori* reason why there should not be many different spatial manifolds in the universe, different in the sense that they have no spatial relations to each other. (It is possible, in addition, that they might have different "geometries"; for example, some might be Euclidian and others not.) Let us go back to dreams again. The images which we experience in a dream are certainly spatial entities. They have extension and shape, and have relations of "near," "far," "between," etc. to one another. But are they *anywhere* in the space of the material world? For example, is a dream-image two inches away from one's left ear, or eighteen inches to the north of the head of one's bed? Such questions, I think, are not only unanswerable, but devoid of meaning. Dream-images are in a space of their own. In relation to the physical world they are nowhere: instead they constitute a "where" of their own; and the only sensible question we can ask is whereabouts a given image is in relation to other dream-images belonging to the same dream. For example, if I dream of a tiger in a jungle my tiger-image is to the left of the palm-tree image which I am aware of at

the same time. We may notice that in discussing his purely dream-like next world, Prof. Ducasse did not ask what spatial relations *it* has to "the material world we now know." And quite rightly. The question just did not arise. Then it should not arise either when we complicate our picture of the dream-like next world by introducing telepathy and clairvoyance, thereby conferring upon it a certain degree of publicity and objectivity. We should just say that such a world (if it should happen to exist) is not *anywhere* in relation to this present material world which we now know, but is in a space of its own.

I am not of course suggesting that Prof. Ducasse ought not to have discussed the reincarnation theory ("survival as transmigration"). I am only complaining of the way he introduces it. And the discussion itself is excellent. All the obvious difficulties in the reincarnation theory are fully considered and ingeniously answered: for example, the lack of memories of previous lives, the fact that our innate mental capacities seem to be derived by physical heredity from our parents, the difficulty of conceiving just *what* it is that is supposed to be reincarnated. Moreover, it seems to me perfectly possible that all the different conceptions of survival which Prof. Ducasse has propounded might be correct. There is no inconsistency between them; and as he suggests himself, they might represent successive stages of after-death experience, with reincarnation in this world (re-embodiment) as the final one. I agree with him that the complex conception of survival which is built up by combining all these different conceptions is at once theoretically possible and "significant" in the sense of satisfying to a rational person. He is quite explicit in saying that he does not claim more for it than this.

All the same, I am inclined to think that in our present state of ignorance discussions of reincarnation are premature. I believe we should be content for the present to show that the notion of *dis*-embodied existence makes sense and that some intelligible content can be given to it. The value of Prof. Ducasse's chapter is that he does succeed in showing this by actually formulating several different but mutually consistent conceptions of what a disembodied mental life might be like, if there is one. Let us be content to stop at that until we have a lot more empirical evidence. The problems

of *re*-embodiment, when and if they arise, may safely be left to our successors.

H. H. PRICE.

THE PSYCHOLOGY OF THE OCCULT. By D. H. Rawcliffe. London: Derricke Ridgway, 1952.

"Everything is here," says the publisher's note, "from 'Auras' to 'Zombies.'" With this five hundred and fifty-one page attack, supported by glossary, bibliography, and index, Mr. Rawcliffe hopes to contribute to "the repulse of the ever advancing tide of supernatural belief which present day psychical research keeps in motion." Psychical researchers may take comfort in the foreword in which Julian Huxley refuses to agree that telepathy and clairvoyance are studies unfit to be admitted to the universities.

The bulk of Mr. Rawcliffe's book is taken up with a description of such matters as mental dissociation, hysteria, hypnotism, hallucination, and trance states. There is nothing new to psychical researchers in the material, but it makes interesting reading, even though it is written in a wordy style unsuited to a presentation of elementary psychology. Examples are given of secondary elaboration in psychic stories and of hysterical dramatization in mediumistic utterances. The evidence for psychic faculties derived from spontaneous and mediumistic cases is given no serious consideration, however, although it is pontifically condemned on almost every other page.

Of special interest to parapsychologists are the four chapters which purport to dispose of ESP experiments. Their tone and content recall the heyday of the ESP controversy, when uninformed critics could impress those more gullible than themselves with murmurs of "fraud" and "sensory cues."

Mr. Rawcliffe begins by quoting some old experiments to show that sensory cues can be transmitted successfully without the process being noticeable to an observer. He cites Tarchanow's work on muscle reading and the Lehmann and Hansen observations on involuntary whispering. In the latter case agent and subject sat back to back facing a pair of concave mirrors. Under these circumstances the tendency to involuntary rudimentary vocalization of the number the agent had in mind was found sufficient to account for the "tele-

pathic" transmission. Six pages are devoted to the case of Ilga K, an illiterate girl who was apparently able to read syllable by syllable whole sentences from a book looked at by her mother. The explanation lay in the words of encouragement constantly used by the mother, who had the habit of tacking onto the end of her exclamations the opening sound of the next word in the book.

The relevance of these observations to the average ESP experiment is doubtful. In his assessment of the role of sensory cues Mr. Rawcliffe shows all the uncritical enthusiasm which he later attributes to Dr. Rhine. There is no real evidence for the existence of true hyperacuity of the senses, and the plausibility of Mr. Rawcliffe's speculations in this direction is not increased by his use of such impressive sounding words as *hyperacusia, hyperaphia,* and *hyperprosexia.* Observations much more pertinent to ESP research than anything quoted by Rawcliffe are to be found in the work of R. M. Collier (*Psychological Monographs,* 1940). In these experiments the subjects were deliberately provided with visual cues. The stimuli were so small as to be below the threshold of conscious discrimination and so unobtrusive that the subjects were unaware of their existence, yet they appeared to be effective in influencing the subjects' guessing to a statistically significant degree.

Mr. Rawcliffe's two biggest bogeys, sensory cues and motivated recording errors, are very simply avoided by using, for clairvoyance tests, pre-recorded decks screened from the view of all present, and by conducting telepathy tests at a distance. Consistent with his practice of ignoring all evidence that does not agree with his theories, Mr. Rawcliffe mentions only two successful distance experiments, Professor Riess's series and Dr. Soal's recent tests between London and Antwerp. The first he "explains" by assuming fraud on the part of the subject or an accomplice, and the second he cannot accept because Dr. Soal himself might have cheated. ESP experimenters can never hope to drive Mr. Rawcliffe back much further than this.

In an intriguing chapter entitled "Criticisms of American ESP Experiments" Mr. Rawcliffe accuses Dr. Rhine of being "dominated by a wholehearted and uncritical enthusiasm." Dr. Rhine's chief fault would appear to be in treating seriously experiments that were not completely proof against the operation of sensory cues. Since there have been plenty of results which sensory cues cannot

explain, Dr. Rhine's attitude on this point is not so unreasonable as it is made to appear. Mr. Rawcliffe notices in the JOURNAL OF PARAPSYCHOLOGY a tendency to stress points favorable to ESP and to gloss over debatable points. This remark, while unfair as a generalization, may have had some justification in particular early reports. Some of Mr. Rawcliffe's other criticisms, however, are hardly more than irresponsible mud-slinging. He abuses investigators for never using mechanical recording, but takes no account of Kahn's experiments in which this was successfully introduced. He quotes against Rhine the fact that Mrs. Garrett, who achieved high scores in the U.S., produced nothing when she was tested in London. He omits to mention that the Americans themselves observed that Mrs. Garrett was failing before ever she came to London. He takes pains to point out that decline effects could be explained if sensory cues were available, but he avoids discussing displacement, reinforcement, and change of call which are more difficult to explain on his theory. He regards the statistical evaluations in ESP reports as unnecessarily erudite mathematical peregrinations that serve to remove attention from the weak points in experimental method. If he had had better insight into the matter, Mr. Rawcliffe might have joined forces with more enlightened critics who would like to see a further refinement of the very elementary statistics commonly employed.

In fact, Mr. Rawcliffe's shortcomings as a critic make his accusations of bias and special pleading in ESP investigators appear peculiarly naïve.

D. J. WEST.

LETTERS AND COMMENTS

To the Editor:

All students of parapsychology are aware that this science works under unique conditions. It has to operate under a double handicap from which every other research has been free in all countries where religious and political freedom has been won. Not only is statistical evidence that would be considered amply convincing in any other study exposed to inquisitorial investigation, but also, as Dr. C. G. Abbot has pointed out, when no flaw can be found in any step of the procedure the figures, though they now approach astronomic proportions, are still maintained to be insufficient to carry conviction.

Not only, then, is the general academic opinion one which, rather than welcoming an interesting discovery, is behaving like a fundamentalist confronted with the Copernican hypothesis. This attitude, as all investigators know, has a highly detrimental effect on research itself. All research is no doubt affected favorably or unfavorably by the intellectual climate in which it is pursued. Certainly during the middle ages a woman or a man possessed of outstanding extrasensory perception must often have been canny enough to have concealed an uncanny gift which would assuredly have brought her or him to the notice of the Inquisition. Even mathematicians and physical researchers such as Roger Bacon went warily; and Bacon, in spite of his caution and papal approval, was imprisoned by his order—his work stopped and himself punished.

The Mechanism of today is as unfriendly to and suspicious of extrasensory perception as was medieval anthropomorphism. The ordinary scientist is as loyal to the mechanomorphic picture of things as was the medievalist to the anthropomorphic. He cannot, however, use imprisonment, torture, and death to suppress such knowledge. Nor would he if he could, so long as he remains a good humanist and so long as the climate of humanitarianism, though now long past its high summer, still prevents the "killing frosts" of fanaticism.

But the fact remains that Mechanism has its own methods whereby it can prevent heresies from getting a hearing or even from being able to produce overwhelming proof of their theories. Three illus-

trations of this which came to the notice of the present writer make this clear. Plenty of others could be cited. The oldest of all the societies for psychical research was looking for a successor to their research officer. It was decided by the Committee that it would be well to choose some student who had taken his degrees in psychology and was proceeding to his doctorate. An eminent head of the Psychology Department of one of the greatest of the universites in Great Britain, when asked if he might be able to assist in such a selection, replied that though it would not influence his own standards of choice, he felt that in the eyes of other electors it would very gravely prejudice the chances of the applicant were it known that he had even investigated the subject of psychical research. Young men are generally not so insensitive to their chances of being able to earn a living that they would feel indifferent to such a climate.

The second occurrence took place in one of the great universities of the West where some young physicists had become interested in extrasensory perception. One of them prepared evidence for a doctorate thesis entitled "Para-physics," and he presented his précis to the three adjudicators appointed. They all allowed that the thesis promised to be an interesting one; but when the problem arose as to which one of the three should act as the chairman, each one of them declined such a dangerous assignment. In consequence the work fell through.

The third example is from a big university of the Middle West. No one on the staff would of course champion such a forbidden subject. One professor (who unfortunately was tainted by an interest in metaphysics) dared to say that the evidence could not be dismissed without further inquiry. The two colleagues (both of the positivistic schools) with whom he was discussing the matter agreed that the whole fantastic superstition must be brought to an end. The more liberal thought that the better way to do this was to let these false and deluded researches go on. Soon, inevitably, it would be evident to all—even to those ignorant of the true difficulty of establishing anything you do not wish to believe by statistics—that the whole investigation was ridiculous. The other felt that the prestige of science with the common man was at stake. If this kind of research went on, science, with all the benefits that it had given to mankind, would be discredited and might be put out of action. Pub-

lic concern—indeed what might be called public morality—required therefore that this false study must be stopped by order. He did not say by whose order nor did he note—perhaps being a specialist he did not know—that the dictators who endow science to serve their purposes and don't fear to repress and kill off those who produce results not ideologically correct, have also always persecuted psychical research. Hitler was savage against it and it is a short road to a Russian concentration camp to study ESP in the U.S.S.R.

There is, then, far too commonly among academic circles an attitude toward ESP that to an outsider seems to violate that belief in the open mind which was once the boast of the university. Empiricism seems to have given way to a priori assumption. Such an attitude would be bound to delay the progress of any study. But with ESP this difficulty is vastly increased. Leo Tolstoy, when he was up against the censorship of the old Tzarist regime, gave classic expression to this situation when he remarked, "It is not what the censor does to the work that I have written that really matters. It is what he does to the work I have yet to write."

The work at Duke and elsewhere has raised psychical research from a field-naturalist study to the rank of laboratory standards. The controls are perfect, the records explicit and undeniable. But the subject, the instrument, the method whereby the researcher investigates and gets results, these are still in the range of art rather than science. We know now from the standard histories of science and the works on the philosophy and techniques of science that there is always an element of art, of insight and, as Einstein has often said, of aesthetic composition in the work of supreme scientists. But we may say that of the three great sectors of research—the inorganic, the organic, and the fields of consciousness—the mechanic pattern works best in the inorganic, at least at molar level. In the organic it is not quite so satisfactory; while in the psychological the idiosyncrasy of the particular case makes generalization increasingly precarious.

Some informed thinkers, we know, have questioned whether psychology in this respect can ever be a science and have therefore defended the tendency of academic psychology to retreat toward psychophysiology. And if this is true in standard psychology how much more must it be so in parapsychology.

We may therefore be cheered and surprised that, considering the opposition that ESP has had to confront and the damage that such unfair opposition can deal to a dawning research, such outstanding results have been obtained. It might have been expected that the contempt and bitter ridicule with which this study has had to begin would totally inhibit evidence being obtained in the laboratory. Then there would have been a permanent chasm between the academic scientist and the field researcher—the one finding results but never able to bring them into the laboratory—the other entrenched in his laboratory charging the field researcher with fraud or ignorance of all scientific method.

There is, then, hope in the examples mentioned above, shocking though they may be to those who imagine that specialist researchers are by their expertism raised above the usual prejudices of human beings. Those who oppose ESP are on the defensive. They would not behave as they do were they really certain of their ground. Research even in the present unfavorable climate is possible and, as it advances, it must change that climate in its own favor. We may hope therefore that what has begun in the exceedingly early and cold spring in which growth must be slow will, in the years that are approaching, find conditions so much more favorable that the output will be far greater, the circumstances under which results can be produced far more general, and that finally we may attain to that state in which whoever wishes to investigate will produce results of significance.

Sincerely yours,

Gerald Heard

GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this JOURNAL, the following definitions are submitted for convenient reference.

A simple description of the basic experimental methods and of the statistical procedures to be followed in evaluating ESP (both card tests and drawings tests) and PK may be found in the *Handbook of Tests in Parapsychology* by Betty M. Humphrey (published by the Parapsychology Laboratory, Duke University, Durham, N. C.).

AGENT: In tests for telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the stimulus object.

AVERAGE SCORE: Average number of hits per run.

BT ("Broken Technique"): The clairvoyance technique in which each card is laid aside by the experimenter as it is called by the subject. The check-up is made at the end of the run.

CALL: The ESP symbol selected by the subject in trying to identify a target.

CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.

CHANCE EXPECTATION = MEAN CHANCE EXPECTATION: The most likely score if only chance obtains.

CHANCE AVERAGE: Mean chance expectation expressed as an expected score, generally in terms of average per run.

CHI SQUARE: A sum of quantities each of which is a deviation squared divided by an expected value. Also a sum of the squares of CR's.

CLAIRVOYANCE: Extrasensory perception of objective events as distinguished from telepathic perception of the mental state of another person.

CR (CRITICAL RATIO): A measure to determine whether or not the observed deviation is significantly greater than the expected random fluctuation about the average. The CR is obtained by dividing the observed deviation by the standard deviation. (The probability of a given CR may be obtained by consulting tables of the probability integral, such as Pearson's.)

CR OF THE DIFFERENCE: The observed difference between the average scores of two samples of data divided by the standard deviation of the difference.

DECK: Twenty-five ESP cards.

CLOSED DECK: An ESP deck composed of five each of the five symbols.

OPEN DECK: An ESP deck made up of the ESP symbols selected in random order.

DEVIATION: The amount an observed number of hits or an average score varies from the mean chance expectation or chance average. A deviation may be total (for a series of runs) or average (per run).

DIE THROW: The elementary unit of data in a dice-throwing test represented by the throwing and reading of a single die, no matter how many dice are thrown at a time.

DISPLACEMENT: ESP responses to targets other than those for which the calls were intended.

BACKWARD DISPLACEMENT: ESP responses to targets preceding the assigned targets (the ones for which they were intended). Displacement to the targets

one, two, three, etc. places preceding the assigned target are designated as (−1), (−2), (−3), etc.

FORWARD DISPLACEMENT: ESP responses to targets coming later than the assigned targets. Displacement to the targets one, two, three, etc. places after the assigned target are designated as (+1), (+2), (+3), etc.

DT (Down Through): The clairvoyance technique in which the cards are called down through the deck before any are removed or checked.

EMPIRICAL CONTROL: An experiment which wholly or partially follows the main experiment with the exception that the conditions are designed to exclude the possibilty of the hypothesis being tested.

ESP (EXTRASENSORY PERCEPTION): Response to an external event not presented to any known sense.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, three parallel wavy lines (called "waves"), square, plus.

ESP SYMBOLS: See plate opposite page 1, this JOURNAL, Vol. 1, March, 1937.

EXPECTATION; see CHANCE.

EXTRACHANCE: Not due to chance alone.

FREE MATERIAL: The stimulus objects of experiments in which an unlimited or unspecified range of stimulus objects is employed (as contrasted with methods such as card-calling in which the subject knows that the stimulus object is one of a known range).

FREE RESPONSE METHOD: An ESP test method in which a relatively unlimited range of stimulus objects is used and in which the subject is instructed to express freely as his response any range of thoughts and ideas.

GESP (GENERAL EXTRASENSORY PERCEPTION): A technique designed to test the occurrence of extrasensory perception, permitting either telepathy or clairvoyance or both to operate.

HIGH-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling eight or more.

LOW-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling six or less.

MEAN CHANCE EXPECTATION; see CHANCE.

P (PROBABILITY): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with those psychical effects which appear not to fall within the scope of what is at present recognized law.

PERCIPIENT: The person who makes the calls in an ESP test.

PK (PSYCHOKINESIS): The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.

PRECOGNITION: Cognition of a future event which could not be known through rational inference.

PREFERENTIAL MATCHING: A method of scoring free responses. A judge ranks the stimulus objects (usually in sets of four) with respect to their similarity to, or association with, each response; and/or he ranks the responses with respect to their similarity to, or association with, each stimulus object.

PSI: A general term to identify personal factors or processes in nature which transcend accepted laws. It approximates the popular use of the word "psychic" and the technical one, "parapsychical."

PSI PHENOMENA: Occurrences which result from the operation of psi. They include the phenomena of both ESP (including precognition) and PK.

QD (QUARTER DISTRIBUTION): The distribution of hits in the record page (or in a logical subdivision thereof, such as the set or the half-set) as found in the four equal quarters formed by dividing the selected unit horizontally and vertically.

RUN: A group of trials, usually the successive calling of a deck of 25 ESP cards or symbols. In PK tests, 24 single die throws regardless of the number of dice thrown at the same time.

SCORE: The number of hits made in one run.

TOTAL SCORE: Total of scores made in a given number of runs.

AVERAGE SCORE: Total score divided by number of runs.

SD (STANDARD DEVIATION): Usually the theoretical root mean square of the deviations. It is obtained from the formula $\sqrt{npq}$, in which n is the number of single trials, p the probability of success per trial, and q the probability of failure. (For ESP cards, SD = 2 $\sqrt{\text{no. of runs.}}$) Sometimes the SD used is that estimated from the observed variability in the scores.

SD OF THE DIFFERENCE: For both ESP cards and PK tests using dice, the SD of the difference is equal to $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where σ_s is the SD of a single run and R_1 and R_2 are the number of the runs in the respective samples compared. This gives the SD of the difference for run score averages.

SERIES: Several runs or experimental sessions that are grouped in accordance with a stated principle.

SESSION: A unit of an ESP or PK experiment comprising all the trials of one test occasion.

SET: A subdivision of the record page serving as a scoring unit for a consecutive group of trials, usually for the same target.

SEVENS TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling seven.

SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. The criterion commonly used in this JOURNAL is a probability value of .02 or less, or a deviation in either direction such that the CR is 2.33 or greater.

SINGLES TEST: A PK technique in which the aim of the subject is to try to influence dice to fall with a specified face up.

STIMULUS OBJECT: The ESP card or drawing or other object, some identifying characteristic of which is to be apprehended by the percipient.

SUBJECT: The person who is experimented upon. In ESP tests, most commonly the percipient (though also the agent in GESP and telepathy). In PK tests, any individual whose task it is to influence the objects thrown.

TARGET: In ESP tests, the stimulus object; or in telepathy, the mental state of the agent. In PK tests, the faces of the die (or combination of faces) which the subject attempts to bring up in the act of throwing.

TARGET CARD: The card which the percipient is attempting to perceive (i.e., to identify or otherwise indicate a knowledge of).

TARGET DECK: The deck of cards the order of which the subject is attempting to identify.

TARGET FACE: The face on the die which the subject tries to turn up as a consequence of direct mental action.

TELEPATHY: Extrasensory perception of the mental activities of another person. It does not include the clairvoyant perception of objective events.

TRIAL: In ESP tests, a single attempt to identify a stimulus object. In PK tests, a single throw of the dice or other objects thrown.

The question of the physical and mental aspects of experience remains. All that can be said is that they are concomitant. If we ask naïvely how the concomitance works, all that can really be said is that we do not know (or with more dignity and less honesty that it is a meaningless question). In this connexion it is interesting to note how many concomitances there are in physics about which no more can be said. Electrons pass from one energy level to another and light quanta appear; mass dissolves and energy traverses space; particles follow statistical laws and are said to be like waves; they are found on the wrong side of barriers too high for them to surmount; space-time is curved in the neighborhood of gravitating bodies. All that is known is that one set of observations will in such cases be relatable to another set of observations of a different kind. To ask for more is according to one view to invent an illusory difficulty, and according to another view to set human intelligence a task which it is not built to carry out. According to a third possible one, it is something which may ultimately be solved by patient progress.

—C. N. Hinshelwood, *The Structure of Physical Chemistry*
(Oxford: Clarendon Press)